**Does the Takeover Market Deter Opportunistic Non-GAAP Reporting?**

Abstract

We exploit the Foreign Investment and National Security Act (FINSA) to examine the effect of an important managerial disciplining mechanism, the takeover market, on the quality of non-GAAP reporting. FINSA significantly reduced the likelihood of takeover for a large fraction of the CRSP-Compustat universe. We draw inferences using a difference-in-differences research design by contrasting non-GAAP disclosures by FINSA-affected firms with those of unaffected firms, before and after FINSA. We find that FINSA-affected firms more often exclude recurring expenses, more often exclude expenses incremental to those excluded by analysts, and more often use non-GAAP earnings to convert a GAAP-based miss of an earnings forecast to a beat, after FINSA. This effect accentuates predictably with the extent to which FINSA-affected firms are susceptible to the takeover market in the pre-FINSA period, and with the extent to which non-GAAP earnings determine manager compensation. We conclude by documenting a decline in non-GAAP earnings persistence and the value-relevance of non-GAAP earnings after FINSA. Our evidence demonstrates the role of the takeover market in curbing opportunistic non-GAAP reporting.

JEL Classification: F52; G14; G34; G38

Keywords: non-GAAP earnings, FINSA, CFIUS, mergers and acquisitions, corporate control.

## 1.0 Introduction

The purpose of this paper is to examine the effect of the market for corporate control on the quality of non-GAAP disclosures. In the US, the practice of disclosing non-GAAP performance measures has grown in frequency since the late 1990s, with over 70% of S&P 500 firms reporting non-GAAP earnings in 2014 (Black et al., 2018b). Proponents of non-GAAP reporting argue that companies use non-GAAP measures to focus investor attention on "core earnings" that exclude non-recurring items and better predict future earnings (Pitt 2001). This view is corroborated by the extent to which the value relevance of non-GAAP earnings exceeds GAAP earnings (Bhattacharya et al., 2003; Elliott, 2006; Ribeiro et al., 2018; Phelps, 2019).

However, critics argue that non-GAAP measures are susceptible to managers' opportunistic exclusion choices. For example, former SEC Enforcement Division Chief Accountant Howard Scheck characterizes non-GAAP metrics as a "fraud risk factor" and a "source of (investor) confusion" (Leone, 2010), one that former SEC chair Mary Jo White argues is worthy of "close attention [from regulators]" (Teitelbaum 2015). Similarly, Lynn Turner, former chief accountant of the SEC, warned investors of the positive bias in non-GAAP earnings and characterized non-GAAP earnings as "EBBS: earnings before the bad stuff".[1]

To address these tensions around the role and merits of non-GAAP reporting, a large body of literature on non-GAAP reporting seeks to determine whether non-GAAP reporting is informative or opportunistic. The consensus in this literature is that non-GAAP reporting is informative to investors, on average. In a recent survey of the extant literature, Black et al. (2018c, p.286) note how non-GAAP reporting quality has improved over the years, and that managers use non-GAAP reporting primarily for "informative reasons." Nonetheless, we know little about how non-GAAP reporting responds to exogenous variation in capital market characteristics. Two exceptions include Bhattacharya et al. (2018) and Christensen et al. (2018) who find that increases in short selling and analyst coverage temper opportunistic non-GAAP reporting.

We add to this literature by examining the effect of the takeover market on non-GAAP reporting. Changes in the takeover market have large implications for management. Theoretical research indicates that an active

[1] Further characterizing non-GAAP reporting as subject to management discretion is a Chief Audit Executive (CAE) at a mid-cap publicly held company who stated that "[t]he way a company treats a non-GAAP measure is indicative of the way management behaves" (personal communication with CAE, March 13, 2019).

takeover market disciplines managers (Jensen, 1993; Scharfstein, 1988; Manne, 1965; Williamson, 1983; Grossman and Hart, 1980; Macey, 1988; Hirshleifer and Thakor, 1998). Empirical evidence indicates a positive (negative) association between the strength of the takeover market and CEO turnover (compensation) (Lel and Miller, 2015; Harford, 2003; Bebchuk, Fried and Walker, 2002; Frattaroli, 2019). This evidence supports a managerial entrenchment hypothesis predicting that managerial opportunism, absent the disciplining effects of an active takeover market, will lead to heightened myopic behavior due to compensation incentives (Healy and Whalen, 1999; Fama and Jensen, 1983; Fama, 1980). Relatedly, a weaker takeover market reduces incentives for private information gathering and thereby increases managers' ability to engage in opportunistic non-GAAP reporting. Takeover premiums are large (Eckbo, 2009) and a reduction in the likelihood of takeover will temper the demand for private information in capital markets (Ferreira and Laux, 2007). Tempered private information-gathering activities may increase information asymmetry between management and outsiders and permit greater opportunism in non-GAAP reporting. Black et al. (2018a) report that non-GAAP earnings are frequently used as a performance metric in compensation contracts. Guest, Kothari and Pozen (2019) further find an association between income-increasing expense exclusions from non-GAAP earnings and higher executive compensation. Overall, the managerial entrenchment hypothesis predicts a weakened takeover market will lead to more opportunistic non-GAAP reporting.

Four countervailing effects suggest the opposite. First, evidence provided by Black et al. (2017b) suggests that accrual-based earnings management is a substitute for opportunistic non-GAAP reporting. Because the takeover market is also associated with increased earnings management (Godsell, 2019), managers may instead shift emphasis to accrual-based earnings management when orchestrating the appearance of improved performance in a weakened takeover market rather than opportunistic non-GAAP reporting.[2] Second, managers concerned with maintaining the pre-FINSA level of disclosure quality may supplant foregone private information-gathering activities by improving the informativeness of non-GAAP reporting. Third, entrenched managers may respond to a reduced likelihood of CEO turnover by indulging in the quiet life (Bertrand and Mullainathan, 2003; Giroud and Mueller, 2010). Managers pursuing the quiet life may engage in fewer high-

[2] See, e.g., Badertscher (2011) and Zang (2012) for a review of trade-offs in earnings management methods.

effort activities such as opening new factories or closing poorly performing factories. The quiet life may have a mechanical effect on managers' ability to engage in opportunistic non-GAAP reporting because the recurring exclusions most commonly characterized as opportunistic are investment and depreciation expenses (Black et al., 2018b). Fourth, managers better protected from the takeover market may have fewer career concerns that would otherwise motivate the orchestration of improved performance through opportunistic non-GAAP reporting. Overall, variation in the takeover market is likely to have countervailing effects on non-GAAP reporting and it is an empirical question whether, and to what extent, the takeover market determines non-GAAP reporting outcomes.

We parse between these competing predictions by exploiting a new source of exogenous variation in the threat of takeover for U.S. firms. The Foreign Investment and National Security Act (FINSA) significantly altered the threat of takeover for a large fraction of the Compustat Universe of U.S. firms deemed important to national security. FINSA provides a powerful setting in which to examine the effects of exogenous variation in the takeover market because FINSA added substantial costs to foreign takeovers of U.S. firms in a subset of industries by spurring an obscure and inactive regulatory committee, the Committee on Foreign Investment in the United States (CFIUS), to scrutinise foreign investment proposals.

Precipitating FINSA was the acquisition of a port management company operating six major U.S. seaports by a state owned company in the United Arab Emirates in February 2006. Spurred by perceived security threats regarding port security, Congressional representatives clamoured for legislative reform of the U.S. foreign investment vetting process. To defuse the situation, the UAE acquirer agreed to sell the newly acquired assets in March 2006 (New York Times, 2006). Nonetheless, Congress and Senate would eventually table over 60 proposed pieces of legislation to address perceived security concerns stemming from foreign investment (Graham and Marchick, 2006). Congress and the Senate debated the legislation that would become FINSA during 2006 and 2007, with the Department of Treasury releasing the first draft of implementing regulations to CFIUS in April 2008.[3]

[3] The final implementing regulations issued in December 2008 were virtually identical to the initially proposed regulations (Rose, 2014).

While ostensibly improving national security, FINSA increased the costs of foreign takeover in four ways. First, by increasing the likelihood of an extended CFIUS investigation of a proposed foreign takeover.[4] Second, by increasing U.S. Congressional involvement in the regulatory approval process and, thereby, political uncertainty for foreign acquirers.[5] Third, by increasing the number of national security-related concessions required from foreign acquirers before takeover approval.[6] Fourth, by increasing the enforcement of, and penalties related to lapses in, foreign acquirer commitments to mitigate ongoing national security concerns.[7]

An ideal feature of the FINSA setting is the availability of a counterfactual. FINSA provides not only an exogenous source of variation in the threat of takeover, but one that primarily affects firms in industries related to national security, while those outside this scope are left relatively untreated. Godsell, Lel and Miller (2019) show that equity values for national-security related firms ebbed and flowed as the strength and severity of FINSA legislation varied during the multi-year legislative process. They conclude that, ultimately, FINSA reduced firm value and takeover likelihood for firms in industries deemed related to national security by CFIUS, while finding little change in equity value and takeover likelihood of firms in industries unrelated to national security.[8] Overall, Godsell et al. (2019) demonstrate that FINSA led to a strong and credibly exogenous decline in the threat of the takeover market for FINSA-affected firms, with little effect on other firms.

---

[4] An investigation subsequent to the 30-day CFIUS review extends the CFIUS process by 45 days. Indicative of these significant changes to the Defense Production Act, the percentage of foreign investment notices investigated by the CFIUS skyrocketed starting in 2007, as shown in Figure 1. Lengthy approval periods are costly for foreign investors because they increase the probability of competing bids (Jarrell and Bradley, 1980).

[5] Bhagwat, Dam and Harford (2016) provide evidence that uncertainty decreases merger and acquisition activity—e.g., "Morgan Stanley said its sale of certain oil-trading and storage businesses to OAO Rosneft may fall apart, as tensions between the U.S. and Russian governments leave the deal in limbo…. Confidence that the sale will ever secure CFIUS's blessing has faded as the U.S. escalated its response to Russia's interference in Ukraine" (Dow Jones News Service, October 10, 2014).

[6] E.g., forcing the target to forfeit sales to the U.S. government or to comply with requests to open books and facilities to authorities without warrant (Byrne, 2015: 877).

[7] E.g., a penalty up to the value of the transaction for mitigation agreement breaches.

[8] At least two caveats are necessary. First, while CFIUS characterizes certain industries as relevant to national security, CFIUS has the purview and authority to examine M&A transactions affecting firms outside the list of industries they provide. This may occur, for example, if the foreign acquirer hails from a country deemed rival to the United States and if the target firm is located adjacent to an undisclosed military base (see, e.g., Ralls vs. CFIUS, 2015 [Li, 2017]), or, if the target firm possesses data that can be used to compromise government or military officials (e.g., the CFIUS forced a Chinese owner to sell Grindr in 2019 in the interests of national security [New York Times, 2019]).

Relatedly, CFIUS is a political entity and politicians may view foreign investment as positive or negative for reasons unrelated to national security, either because of the nature of the foreign acquirer or because of the nature or location of the target firm. Consequently, CFIUS may scrutinize industries outside of industries listed in the 2008 Annual Report if co-opted by politicians. Our definition of treatment will suffer from measurement error if CFIUS scrutinizes industries other than those industries listed in the 2008 CFIUS Annual Report. Such a confound works against our predicted findings because the population of treated firms in the control group will narrow the gap in observed differences between treatment and control before and after FINSA.

Emulating their research design, we examine firm outcomes for FINSA-affected firms after FINSA, relative to firms largely unaffected by FINSA, to examine the impact of the takeover market on the quality of non-GAAP reporting. We use a difference-in-differences (DiD) analysis that examines non-GAAP reporting in FINSA-affected firms (treatment) and non-FINSA affected firms (control) before and after the enactment of FINSA. A DiD estimator showing that FINSA-affected firms more often exclude recurring expenses from non-GAAP earnings, relative to control firms, provides our first evidence on the effect of the takeover market on the quality of non-GAAP reporting. Second, we demonstrate that the magnitude of recurring exclusions increases after FINSA for FINSA-affected firms, relative to control firms. Third, we find that FINSA-affected firms more often exclude expenses from non-GAAP earnings that analysts do not. Fourth, we find that the gap by which manager exclusions exceed analysts exclusions grows for FINSA-affected firms after FINSA, relative to control firms. Fifth, we find that FINSA-affected firms more often use non-GAAP earnings to beat analyst benchmarks when GAAP earnings fall short. In subsequent analysis, we provide corroborating evidence of our channel (the takeover market) as well as a mechanism (executive compensation) by showing that these effects are predictably concentrated in: 1) firms which had a higher probability of takeover pre-FINSA; and 2) firms whose managers' compensation is more sensitive to non-GAAP earnings. We further demonstrate that FINSA-affected firms' change in non-GAAP reporting behavior reduced the power of non-GAAP earnings to predict future financial performance. We conclude with evidence that investors recognize the shift in managers' non-GAAP reporting incentives, as evidenced by a decline in the value-relevance of FINSA-affected firms' non-GAAP earnings after FINSA, relative to control firms.

Overall, our results show that financial protectionism has important financial reporting consequences. Our evidence supports the managerial entrenchment hypothesis by showing that a weakened takeover market degrades the quality of non-GAAP reporting. These insights are relevant to non-GAAP scholars, investors, analysts, auditors, board directors and regulators interested in the determinants of non-GAAP reporting quality and to audiences interested in the unintended consequences of regulating foreign investment. Our findings should be of interest to legislators in the U.S. where CFIUS authority has been expanded, under the Foreign

Investment Risk Reduction and Management Act of 2018, to include foreign investment in start-ups,[9] joint ventures and greenfield foreign investment (Rose, 2018). It should also be of interest to legislators in the increasing number of countries that are considering or have recently passed FINSA-like legislation where reporting of non-GAAP earnings is proliferating (e.g., Britain, France, Germany, China, and India [Steinitz and Ingrassia, 2009; CNN Business, 2018]). The rest of this paper is as follows. We describe the FINSA setting, the prior literature, and develop our hypotheses in Section 2. In Section 3, we describe our sample and research design. Section 4 presents our empirical results. Section 5 concludes.

## 2.0 Institutional background and hypothesis development

### 2.1 A primer on the FINSA setting

The origins of CFIUS are rooted in the Defense Production Act of 1950, which permits the U.S. President to reject foreign investments that threaten national security. To allow the administration to adapt to evolving forms of national security threats, national security is deliberately undefined in legislation and regulatory documents. Moran (2009), however, describes three potential national security threats posed by foreign acquirers. First, excessive reliance upon foreign-owned enterprises could render defense contractors vulnerable to supply chain disruptions. Second, acquired technology could be deployed by the acquirer for other than commercial and financial purposes, potentially enabling U.S. rivals. Third, the acquired entity could be used as a conduit or channel through which foreign entities could inhibit U.S. interests (e.g., through surveillance, infiltration and sabotage).

President Ford delegated the review of foreign transactions to CFIUS in 1975. Before FINSA, the outcomes of CFIUS reviews were nearly always affirmative and injected few political uncertainties into the acquisition process for domestic targets and foreign acquirers (Byrne, 2006). In contrast, by enacting FINSA in 2008 to amend the Defense Production Act, Congress significantly increased CFIUS's ability to thwart foreign acquisitions. FINSA charges CFIUS with scrutinizing foreign investments for patterns of coordinated

[9] While it is too early to empirically assess the effects of FIRRMA, the effect of CFIUS on foreign investment in start-ups is already anecdotally observable. For example, On April 5, 2019, CFIUS ordered iCarbon X, a Chinese company to divest its majority stake of U.S. start-up, PatientsLikeMe, due to national security concerns.

acquisition behavior that could result in the transfer of advanced technologies to U.S. rivals. Three times since 2008, the CFIUS has concluded that

> …the United States Intelligence Community believes that there may be an effort among foreign governments or companies to acquire U.S. companies involved in research, development, or production of technologies for which the United States is a leading producer. (CFIUS, 2012; p. 23; 2014; p.26; 2015; p. 29)

In strengthening CFIUS, FINSA increased the barriers to foreign investors in four main ways. First, FINSA delays the acquisition timeline by increasing the purview of CFIUS and, thus, the likelihood of not only a month-long "review" process but also a second 45-day CFIUS "investigation." Lengthy approval periods are costly for would-be acquirers because they increase the probability of competing bids (Jarrell and Bradley, 1980).

Second, FINSA increases deal uncertainty by injecting additional political risk for foreign acquirers. FINSA gives Congress a larger role in the investment approval process, adding to the uncertainty of the President's approval. Prior research shows that uncertainty decreases investment activity (Bonaime, Gulen and Ion, 2018; Jens, 2017; Bhagwat, Dam and Harford, 2016). Uncertainty is further accentuated because all details of CFIUS investigations are kept confidential. CFIUS is exempt from FOIA, so there are no disclosures of even basic information, such as the name of the potential foreign acquirer or domestic target.

Third, FINSA increases the likelihood that foreign investors are burdened with costly national security risk mitigation agreements. For example, mitigation agreements may require that the acquirer terminate specific activities of the U.S. business being acquired or provide the U.S. government with the right to review certain business decisions and object if they raise national security concerns. Appendix A provides a comprehensive list of economically burdensome risk mitigation clauses. Consistent with risk mitigation costs being large, foreign investors frequently abandon their proposed deal after CFIUS proposes risk mitigation terms. Other would-be foreign acquirers simply withdraw their offers after learning that the CFIUS would review the deal.[10]

Fourth, FINSA strengthens CFIUS enforcement and penalties for noncompliance. FINSA provides for the "imposition of civil penalties for any violation…, including [violations of] any mitigation agreement" (p. 97). FINSA also encourages CFIUS to "develop and agree upon methods for evaluating compliance with any

[10] For example, "Tsinghua Unisplendour, a Chinese state-controlled company, dropped plans to buy 15% of Western Digital, an American maker of computer hard-drives, for $3.8 billion. The Chinese withdrew after the Committee on Foreign Investment in the United States, a government body, said it would review the deal." The Economist (Espresso), February 24th, 2016.

agreement entered into or condition imposed with respect to a covered transaction that will allow the Committee to adequately assure compliance," which bolsters their sanctioning authority (p. 97). Rose (2014) notes that "FINSA allows CFIUS to reopen reviews and investigations if there has been an intentional breach of a mitigation agreement" (p. 32). After FINSA, the breaking of a mitigation agreement can result in monetary penalties up to the full value of the transaction and may potentially force the unwinding of the transaction (Rose, 2014; p. 14).

**2.2 Non-GAAP Literature Review**

Investors pay more attention to non-GAAP earnings than GAAP earnings when allocating capital. Bhattacharya et al. (2003) were the first to compare the informativeness of GAAP earnings with non-GAAP earnings using a hand-collected non-GAAP disclosure sample. The authors found that investors and analysts reacted more to non-GAAP earnings news than to GAAP earnings news. Elliott (2006) finds that experiment participants base trading decisions primarily on non-GAAP earnings highlighted by the firm in its earnings announcement. Ribeiro et al. (2018) find that non-GAAP earnings are more value-relevant for Australian firms. Phelps (2019) finds that both non-GAAP earnings and funds flow from operations (a non-GAAP cash flow figure) are more informative to investors than GAAP earnings.

Because investors rely on non-GAAP earnings when forming prices, managers can use discretion in non-GAAP earnings to improve investor understanding of firm performance, but they may also opportunistically use discretion in non-GAAP earnings to orchestrate the appearance of higher performance. Much of the non-GAAP literature parses between these competing determinants of non-GAAP reporting with increasing evidence that non-GAAP reporting is informative to investors, on average (Black et al., 2018c). For example, Lougee and Marquardt (2004) find that firms with low GAAP earnings informativeness are more likely to disclose non-GAAP earnings. Curtis, McVay and Whipple (2014) examine firms that report net transitory gains (income-increasing special items) in some periods and net transitory losses in others. They find that the proportion of informative disclosers (firms that report non-GAAP earnings in both situations) is significantly higher than the proportion of opportunistic disclosers (those who report non-GAAP earnings only when there are net transitory losses). Other studies find improving non-GAAP reporting quality of over time. Kolev et al.

(2008) find that non-GAAP expense exclusions are more transitory (i.e., higher quality) after SEC intervention in non-GAAP reporting between 2001 and 2004. Zhang and Zheng (2011) find that stock mispricing for non-GAAP reporters drops off after Regulation G. Black et al. (2017a) find that firms' non-GAAP exclusions are less opportunistic in the post-SOX period.

However, other studies document the appearance of opportunism in non-GAAP reporting. Bhattacharya et al. (2003) demonstrate that non-GAAP earnings exceed GAAP earnings 70% of the time, are positive 65.5% of the time (versus 52.0% for GAAP operating earnings from Compustat) and meet or beat analyst forecasts 80% of the time (versus 39% of the time based on GAAP operating earnings). Doyle et al. (2003) find that the non-GAAP exclusions outside of special items have nearly all the predictive power of analyst-defined street earnings, but in the negative direction.[11] This shows that managers' exclude recurring expenses from non-GAAP earnings despite Regulation G, which forbids firms from excluding charges and gains that have occurred within the previous two years or is likely to recur within two years. Barth et al. (2012) find that the predictive ability of non-GAAP earnings falls when firms exclude stock-based compensation expense post-SFAS 123R. Brown et al. (2012) find that managers' non-GAAP exclusions have a greater negative relationship with future operating income prior to insider share sales, suggesting opportunistic non-GAAP reporting.

Few papers examine the impact of distinct market forces on the quality of non-GAAP reporting. Christensen et al. (2014) find that short sellers appear to target opportunistic non-GAAP reporters and Bhattacharya et al. (2018) shows that the threat of short selling curbs firms' opportunistic non-GAAP disclosures. Christensen et al. (2018) report that exogenous declines in analyst coverage cause managers to report non-GAAP earnings more opportunistically. Overall, this literature suggests that while non-GAAP reporting is informative on average, attenuation in managerial disciplining mechanisms and the information environment leads to increases in opportunistic behavior.

**2.3 Hypothesis Development**

[11] If the exclusions have negative predictive power for future earnings that means the exclusions include recurring (rather than non-recurring) expenses. Assuming the purpose of non-GAAP reporting is to present "core earnings", these recurring expenses should not be excluded as it causes investors to underestimate future expenses.

The discussion in Section 2.1 identifies FINSA as a source of exogenous variation in the costs borne by foreign acquirers of treatment firms. A nascent literature detects associations between these costs and the frequency and magnitude of foreign takeover activity (Godsell, Lel and Miller, 2019). Based on evidence of a significant decline in the takeover market following FINSA for FINSA-affected firms, we contribute to the literature described in Section 2.2 by investigating a new determinant of non-GAAP reporting.

We present a null hypothesis acknowledging the myriad channels by which the takeover market may effect non-GAAP reporting. Management entrenchment theory predicts that myopia will increase after a negative shock to the takeover market because the takeover market is a managerial monitoring mechanism (Manne, 1965). An attenuated takeover market grants managers leeway to extract rents from the firm because the takeover market is a managerial disciplining mechanism that spurs private information gathering activities that would otherwise curb managerial opportunism (Ferreira and Laux, 2007). Absent an active takeover market, managers may more often engage in opportunistic non-GAAP reporting because non-GAAP earnings are a determinant of executive compensation (Black et al., 2018a; Guest, et al., 2019).

In contrast, increasing use of accrual-based earnings management after FINSA by FINSA-affected firms (Godsell, 2019) may supplant non-GAAP earnings as a tool managers use to orchestrate the appearance of improved operating performance and reduce opportunism in non-GAAP reporting (Black et al., 2017b). Alternately, quiet life theory (Bertrand and Mullainathan, 2003; Giroud and Mueller, 2010) predicts that a weakened takeover market will increase the informativeness of non-GAAP reporting through the negative effect of reduced investment activity on the supply of the investment and amortization accruals typical of those recurring expenses that managers opportunistically exclude from non-GAAP earnings. Furthermore, managers with fewer career concerns and pressures to orchestrate the appearance of improved operating performance could also drive a reduction in opportunistic non-GAAP reporting. Yet further, signalling theory suggests that managers may increase the informativeness of non-GAAP reporting to substitute for the reduction in private information gathering following the reduction in the takeover market activity. This theory predicts that market participants are cognizant of the managerial disciplining role played by the takeover market and will take steps to protect themselves if the takeover market is weakened. Managers keen to pre-empt this investor response

will improve the informativeness of non-GAAP reporting. Overall, we expect variation in the takeover market to either increase or decrease opportunistic non-GAAP earnings reporting. Formally stated:

**$H_0$**: Non-GAAP reporting quality does not change after FINSA for FINSA-affected firms, relative to control firms.

## 3.0 Data and Research Design

### 3.1. Data

Treatment firms are firms in the CFIUS list of industries deemed relevant to national security. Firms that fall within the definition of our treatment group provide "products and services to an agency or agencies of the U.S. government, or state or local authorities that have functions that are relevant to national security" (CFIUS Annual Report, 2015, p. 23).[12] Appendix B provides a listing of qualitative attributes characterizing U.S. firms scrutinized by CFIUS. We form our DiD estimator by interacting our treatment variable, *Treatment Industry*, with our post variable, *PostFINSA,* which is an indicator variable equal to one in the twelve quarters after the second quarter of 2008, and zero in twelve quarters prior to March 2006: the month of the acquisition event that precipitated FINSA. We exclude the legislative period spanning March 2006 to June 2008 due to the legislative process' conflicting market signals prior to the eventual adoption of CFIUS (Graham and Marchick, 2006; Godsell et al., 2019).

Our tests use quarterly data. We draw firm-level financial statement data for our sample period from Compustat Fundamentals Quarterly File. Table 1 shows firm-year observations at each stage of the data screening process, starting with data retrieval and ending with the estimation sample. We start with 15,767 firm-quarter observations in the intersection of Compustat, CRSP and I/B/E/S that report non-GAAP EPS (Bentley et al., 2018). We next remove firms with missing test and control variables, firms without data in both

[12] Treatment industries are defined using industries listed in the 2008 CFIUS Annual Report to Congress (Public Version), which approximates the "Militarily Critical Technologies List" curated by the U.S. Department of Defense. Listed industries fall under the categories Advanced Materials and Processing, Chemicals, Advanced Manufacturing, Information Technology, Telecommunications, Microelectronics, Semiconductor Fabrication Equipment, Electronics: Military Related, Biotechnology, Professional/Scientific Instruments, Aerospace and Surface Transportation, Energy, Space Systems, and Marine Systems.

the pre- and post-FINSA period and firms in regulated industries.[13] The estimation panel we use to test $H_0$ includes 6,100 (658) firm-quarters (distinct firms).

**Insert Table 1 about here**

The DiD tests are formed using FINSA and non-FINSA affected firms. Approximately half (2,877) of estimate sample observations are FINSA-affected. Consistent with Godsell (2019) and Godsell, Miller and Lel (2019), Panel A of Table 2 shows that approximately two-thirds of treatment firms are in high-tech industries. We offer a year-quarter breakdown in in Panel B of Table 2. We observe a larger number of observations in the post period, consistent with the trend of increased use of non-GAAP reporting over time. To ensure that we are examining the same group of firms in the pre and post period, sample firms included in the post period are required to issue non-GAAP disclosures in at least one firm-quarter observation in the pre-period.

**Insert Table 2 about here**

We report summary statistics for variables included in the foregoing equations in Table 3. We find that our sample firms are profitable, with average (non-) GAAP EPS of ($0.41) $0.20 for control firms, and ($0.29) $0.12 for treated firms. These figures are in line with prior research, with Black et al. (2017a) reporting average (non-) GAAP EPS of ($0.35) $0.23 in their post-SOX sample. Firms in our sample (1) are large, with average total assets of $5.9B ($4.4B) for control (treated) firms; (2) are efficient, with average ROA of 0.08 (0.06) for control (treated) firms; and (3) priced favorably by investors, with average Market-to-Book of 2.56 (2.88) for control (treated) firms. Most firms in the sample are audited by a Big-4 audit firm (94% of control firms, 93% of treated firms) and have somewhat variable operating cash flows (3.7% of assets for control firms, 4.8% for treated firms). Investor sentiment was negative over the sample period, on average (-0.29 for control and treated firms). Compared to treated firms, control firms have somewhat less analyst following (9.9 analysts vs. 11.3), are more highly levered (debt-to-asset ratio of 0.55 vs. 0.40), and share the same proportion of institutional ownership (76%). Overall, control firms are repeat non-GAAP reporters less often than treated firms (57% vs.

[13] There is notable variation in the definition of regulated industries in the non-GAAP literature. We test the sensitivity of our results to this research design choice in Section 5.

69%), with similar rates of special charges (63% vs. 65%). A difference-in-means test across treatment and control firms motivates our inclusion of our control variables in the model we estimate.

Sample firms are very likely to exclude expenses from non-GAAP earnings, with 77% (87%) of control (treated) firms reporting higher non-GAAP EPS than GAAP EPS. Control (treated) firms exclude recurring expenses 61% (81%) of the time, and non-recurring expenses 59% (56%) of the time. These rates of exclusions are similar but somewhat higher than those reported in the Bhattacharya et al. (2018) estimation sample used that ends in 2007. In the mean, as a percent of total assets per share, control (treated) firms' total excluded items amount to 1% (1.5%), while recurring items amount to <0.5% (>0.5%) and non-recurring items >0.5% (>0.5%). Both control and treated firms exclude expenses incremental to those excluded by analysts at a rate of 11% of the time, with these exclusions amounting to about 0.1% of total assets per share, on average. In terms of besting analyst estimates, control (treated) firms' non-GAAP EPS meets or beats the consensus forecast, while GAAP EPS falls short, 40% (55%) of the time. This frequency is slightly higher than that reported in Christensen et al. (2018) who again use a different sample period.

**Insert Table 3 about here**

### 3.2. Research Design

To determine the effect of the takeover market on deterring opportunistic non-GAAP earnings, we estimate the general model described by Equation (1):

$$\textit{Non-GAAP Reporting Outcome}_{it} = B_0 + B_1\textit{Treatment Firm}_{it} + B_2\textit{PostFINSA}_{it} + B_3\textit{Treatment Industry}_{it} \times \textit{PostFINSA}_{it} + B_{4\text{-}15}\textit{Controls}_{it} + \lambda_k + \tau_t + \varepsilon_{it} \quad (1)$$

where *Non-GAAP Reporting Outcome* is defined by one of several alternative non-GAAP reporting variables, $\lambda_k$ is a set of industry fixed effects and $\tau_t$ is a set of year-quarter fixed effects. Our outcome variables include *Manager Total Exclusions Indicator*, *Manager Recurring Exclusions Indicator*, *Manager Non-Recurring Exclusions Indicator*, *Manager Total Exclusions*, *Manager Recurring Exclusions*, *Manager Non-Recurring Exclusions*, *Manager Incremental Exclusions Indicator*, *Manager Incremental Exclusions* and *StreetBeat*. We define all variables in Appendix C. We employ a probit model when the dependent variable is equal to either zero or one, and an OLS model when the dependent variable is continuous. We cluster standard errors by firm in all models. If the takeover market

deters opportunistic non-GAAP reporting, we expect to find a positive coefficient on $B_3$ because FINSA weakens the takeover market (Godsell et al., 2019).

We also include a battery of control variables prior research has found to influence firms' non-GAAP reporting (Heflin and Hsu, 2008; Brown et al., 2012; Bhattacharya et al., 2018; Christensen et al., 2018). We control for firm size (*Size*), profitability (*ROA*), valuation (*Market-to-Book*), audit quality (*Big 4 Indicator*), cash flow volatility (*Standard Deviation of Cash Flows*), investor sentiment (*Investor Sentiment*), institutional ownership (*Institutional Ownership*), analyst coverage (*Number of Analysts Following*), leverage (*Book Leverage*), non-GAAP reporting history (*Repeat non-GAAP Reporter*), the expensing of special items (*Special Charges*), and a fourth-quarter indicator (*Fourth Quarter Indicator*). Heflin and Hsu's (2008) results inform our prediction for a negative coefficient on *Size* and a positive coefficient on *Fourth Quarter Indicator*. Based on Christensen et al. (2018), we predict negative coefficients on *ROA, Standard Deviation of Cash Flows* and *Book Leverage*, and positive coefficients on *Number of Analysts Following* and *Institutional Ownership*. The results of Bhattacharya et al. (2018) lead us to predict a negative coefficient on *Market-to-Book* and positive coefficients on *Special Charges* and *Repeat non-GAAP Reporter*. Brown et al.'s (2012) study motivates a negative prediction on the coefficient for *Big 4 Indicator* and a positive coefficient on *Investor Sentiment*.

## 4.0. Results

### 4.1 DiD Results

#### *4.1.1 The effect of the takeover market on non-GAAP Earnings*

To determine whether the takeover market deters opportunistic non-GAAP earnings we first examine the frequency of recurring and non-recurring non-GAAP exclusions after FINSA for FINSA-affected firms relative to control firms. We start by testing the overall relationship between a weakened takeover market and non-GAAP exclusions in Table 4. The test variable and DiD estimator is *Treatment Industry* × *Post-FINSA*. In Column (1) we find a positive and statistically significant coefficient on our DiD estimator which documents an increasing frequency of non-GAAP income-increasing expense exclusions in non-GAAP earnings after FINSA. This coefficient suggests a 5.2% marginal increase in the probability of treated firms excluding expenses from

non-GAAP earnings after FINSA.[14] To distinguish between informative and opportunistic non-GAAP expense exclusions, we decompose non-GAAP exclusions into recurring and non-recurring exclusions. If a weakened takeover market causes an increase in opportunistic non-GAAP exclusions, then we will observe a positive and statistically significant coefficient on *Treatment Industry* × *Post-FINSA* in Column (2) where we replace our outcome variable with *Manager Recurring Exclusions Indicator*. It is also possible that a correlated omitted variable somehow concurrently affects both recurring and non-recurring non-GAAP exclusions reported by FINSA-affected firms, after FINSA. If this is the case, we will observe a positive and statistically significant coefficient on *Treatment Industry* × *Post-FINSA* in Column (3) where we replace our outcome variable with *Manager Non-Recurring Exclusions Indicator*.

In Column (2), we find a positive and statistically significant coefficient on our DiD estimator, *Treatment Industry* × *Post-FINSA*. This variable suggests that our measure of opportunistic exclusions, recurring exclusions, increased after FINSA for FINSA-affected firms relative to control firms. The marginal effect of the DiD estimator is an economically significant 14.6% increase in the probability of recurring expense exclusion, relative to the probability of recurring expense exclusion across all firms.[15] This is our first empirical evidence rejecting our null hypothesis, $H_0$. In Column (3), we find a negative coefficient indistinguishable from zero on the DiD estimator. This result suggests that FINSA-affected firms did not change the frequency of non-recurring exclusions after FINSA relative to control firms.

Control variable coefficients tabulated in Column (1) corroborate the prior literature. As predicted by Heflin and Hsu (2008), we find a negative coefficient on *Size* and *Fourth Quarter*. As predicted by Christensen et al. (2018), we find negative coefficients on *ROA, Standard Deviation of Cash Flows,* and *Book Leverage*, and positive coefficients on *Number of Analysts Following*. As predicted by Bhattacharya et al. (2018), we find a positive coefficient on *Special Charges* and *Repeat Non-GAAP Reporter*. Our ability to replicate these findings adds support to the robustness of results reported in the prior literature. The results in Table 4 provide our first contribution

[14] The coefficient on Treatment Industry × Post-FINSA from Column (1) of Table 3 represents a 4.3% marginal increase in probability of expense exclusion from non-GAAP earnings. This is a 5.2% increase in probability relative to the rate of expense exclusion of all firms (82.2%).
[15] The marginal effect of a 10.3% increase in probability represents a 14.6% relative increase (sample mean = 70.3%).

to this literature by demonstrating that a weakened takeover market is associated with an increase in opportunistic non-GAAP disclosures.

**Insert Table 4 about here**

To corroborate our initial empirical evidence, we next examine the effect of the attenuation in the takeover market caused by FINSA on the magnitude of non-GAAP exclusions. In Table 5, as in Table 4, we first examine all exclusions and then exclusions decomposed into recurring and non-recurring exclusions. Similar to Table 4, we observe a positively but statistically marginal coefficient on our DiD estimator, *Treatment Industry* × *Post-FINSA*, in Column (1). Our Table 4 evidence is corroborated by the positive and statistically significant coefficient on the DiD estimator in Column (2) of Table 5. This coefficient translates into an economically meaningful 41.1% increase in the magnitude of recurring expenses excluded from non-GAAP earnings.[16] In Column (3) of Table 5, we continue to find a DiD estimator indistinguishable from zero in tests examining the effect of the takeover market on non-recurring exclusions.

**Insert Table 5 about here**

It remains possible that exclusions anticipated to be non-recurring recur despite bona fide estimates by management. To examine whether the increase in recurring exclusions we observe in FINSA-affected firms after FINSA are errors rather than opportunistic, we juxtapose the exclusions reported by management with the exclusions recorded by IBES analysts. We posit that the recurring exclusions we observe may be errors that are best estimates based on the information available at the time if both managers and analysts record similar exclusions. The intuition is that recurring exclusions that are not similarly recorded by analysts are opportunistic. We estimate Equation (1) after replacing the outcome variable with an indicator equal to one if there is a gap between managers' and analysts' estimates of exclusions in Table 6. We find a positive and statistically significant coefficient on our DiD estimator, *Treatment Industry* × *Post-FINSA*. The marginal effect is substantial, suggesting that after FINSA, FINSA-affected firms are 46.8% more likely to exclude more expenses from non-GAAP

[16] The marginal effect of an increase in recurring exclusions of 0.2% of total assets represents a 41.1% increase in recurring exclusions relative to the sample mean of 0.5%.

earnings than analysts are willing to incorporate into their estimates.[17] This is additional evidence of a negative association between the takeover market and the opportunism in non-GAAP reporting.

**Insert Table 6 about here**

We search for corroboration of this inference by regressing the magnitude of the wedge between managers and analysts estimates of exclusions in Table 7. We regress the outcome variable, *Managerial Incremental Exclusions,* on our DiD estimator, *Treatment Industry* × *Post-FINSA*. We report the results in Table 7. The DiD estimator, *Treatment Industry* × *Post-FINSA*, is positive and statistically significant, suggesting a 86.9% increase in the magnitude of expenses excluded by managers in excess of those excluded by analysts.[18] This test further corroborates the negative association between the takeover market and opportunism in non-GAAP reporting.

**Insert Table 7 about here**

*4.1.2 The effect of the takeover market on Miss-to-Beat conversion*

We conclude our main empirical analysis with tests investigating the extent to which managers use exclusions to orchestrate the appearance of analyst earnings forecast achievement. We regress the outcome variable, *StreetBeat*, on our DiD estimator, *Treatment Industry* × *Post-FINSA*. *StreetBeat* is an indicator variable equal to one if GAAP earnings miss an analyst earnings forecast while non-GAAP earnings meet or beat the analyst earnings forecast, and zero if 1) the inverse, 2) if neither beat, or 3) if both meet or beat analyst earnings forecast. We report the results in Table 8. The DiD estimator coefficient is positive and statistically significant, consistent with FINSA-affected firms more often using non-GAAP exclusions to convert a miss to a beat after FINSA, relative to control firms. This coefficient represents a 49.0% marginal effect on the probability of turning a miss based on GAAP earnings into a meet or beat based on non-GAAP earnings.[19] This evidence further corroborates earlier evidence of increased opportunistic non-GAAP exclusions after a decline in the strength of the takeover market.

**Insert Table 8 about here**

[17] The marginal effect of a 5.3% increase in probability represents a 46.8% relative increase (sample mean = 11.3%).
[18] The marginal effect of an increase in incremental exclusions of 0.07% of total assets represents an 86.9% increase in recurring exclusions relative to the sample mean of 0.08%.
[19] The marginal effect of a 15.8% increase in probability represents a 49.0% relative increase (sample mean = 32.3%).

### 4.2 Cross Sectional Results

*4.2.1 Intensity of takeover threat*

The results of this study suggest that a decline in the potency of the takeover market causes managers to engage in more opportunistic non-GAAP reporting. If it is the weakening of this managerial disciplining mechanism that is causing managers to alter their voluntary disclosure strategies, we should observe a relative concentration of this effect in firms with a higher exposure, before the post-FINSA period, to takeover risk. This lends itself to a cross-sectional analysis examining firms with differing levels of pre-FINSA takeover likelihood. We estimate pre-FINSA takeover likelihood using the linear probability model in Equation (2):

$$Takeover_{iy} = B_0 + B_1 Size_{iy} + B_2 Leverage_{iy} + B_3 Market\text{-}to\text{-}Book_{iy} + B_4 PropRatio_{iy} + B_5 Liquidity_{iy} + B_6 Growth_{iy} + B_7 Performance_{iy} + B_8 RET_{iy} + B_9 HHI_{iy} + \lambda_k + a_y + \varepsilon_{iy} \quad (2)$$

where $Takeover_{iy}$ is equal to one if an acquirer purchases more than 50% of firm equity during the year at a cost in excess of $5 million. Karpoff et al. (2017) and Godsell et al. (2019) motivate our choice of firm-level control variables for Equation (2). Control variables including size, book leverage, market-to-book ratio, property ratio, liquidity ratio, sales growth, return on assets, market returns and HHI are defined in Appendix C. $\lambda_k$ is a set of industry fixed effects, $a_y$ is a set of year fixed effects. We cluster standard errors by firm.

After estimating this regression for fiscal years 2005-2008, we average the firm-year predicted values by firm to obtain the firm-specific predicted probabilities of takeover, then calculate their median value across the sample.[20] Firms with predicted probabilities greater than the median face a higher exposure to takeover risk than firms with predicted probabilities below the median. We use this partition to estimate Equation (1) in the cross section, investigating how the impact of FINSA on non-GAAP reporting varies with the intensity of firm-specific takeover risk. We report the outcome in Panel A of Table 9. We find results consistent with a concentrated effect on firms with greater exposure to takeovers. This evidence corroborates FINSA's effect on

[20] The choice of years to include in the takeover prediction model requires a trade-off between sample size and sample relevance. Keeping the estimation period as close as possible to the post-FINSA period, where we observe non-GAAP reporting outcomes, is critical as takeover activity in this period is more closely associated with post-FINSA expected takeover probabilities than is takeover activity in previous periods. Including other years from our period of study (2004 or 2003-04) in the takeover prediction model yields qualitatively similar cross-sectional results and unchanged inferences.

the takeover market as the channel for the effect we observe because the effect is strongest for those firms most likely to be subject to the disciplining forces of the takeover market.

**Insert Table 9 about here**

*4.2.2 The role of executive compensation*

Prior research demonstrates that non-GAAP earnings are frequently used as a performance metric in executive compensation contracts.[21] We expect managers for whom non-GAAP earnings is a stronger determinant of compensation to engage in more opportunistic non-GAAP reporting after FINSA weakens the takeover market. To test our prediction, we first calculate the sensitivity of CEO compensation to non-GAAP earnings by estimating Equation (3) by industry:

$$Compensation_{iy} = B_0 + B_1 NGE_{iy} + B_2 GAAPIB_{iy} + B_3 GAAPOI_{iy} + B_4 Returns_{iy} + B_5 REV_{iy} + B_6 BTM_{iy} + B_7 Tenure_{iy} + a_y + \varepsilon_{iy} \quad (3)$$

where $Compensation_{iy}$ is equal to the natural log of total compensation and $NGE_{iy}$ is equal to the natural log of annualized non-GAAP earnings. All variables are defined in Appendix C. $a_y$ is a set of year fixed effects and standard errors are clustered by firm. The choice of firm characteristics controlled for in the compensation model is motivated by previous literature (Guest et al., 2019).[22]

After estimating this regression by industry for fiscal years 2005-2008, we calculate the median value of $\widehat{B_1}$ across the sample.[23] Industries with $B_1$ estimates greater than (below) the median consist of firms with a higher (lower) sensitivity of executive compensation to non-GAAP earnings. We use this partition to estimate Equation (1) in the cross section, investigating how the impact of FINSA on non-GAAP reporting varies with the sensitivity of executive compensation to non-GAAP earnings. We report the outcome in Panel B of Table 9. We find results consistent with a concentrated effect on firms with managers for whom non-GAAP earnings

[21] Black et al. (2018a) and Guest et al. (2019) also report that the non-GAAP earnings figure reported to investors in earnings announcements is the same as the figure reported as a compensation determinant in proxy statements in most cases, with any differences being "generally small" (Guest et al., 2019).

[22] We also follow Guest et al. (2019) by setting $NGE_{iy}$ to zero if annualized non-GAAP earnings is negative.

[23] We require at least 20 observations per industry-level regression. This restriction is in place to ensure consistent estimates of $B_1$. Results are qualitatively similar, and inferences are unchanged without this restriction or with other restriction levels, e.g., a minimum of 15, 25, or 30 observations.

is a stronger determinant of compensation. This is consistent with managers capitalizing on the decline in the takeover market by opportunistically reporting non-GAAP earnings in order to increase their compensation.

**4.3 Persistence Tests**

The results from section 4.1 suggest that managers of FINSA-affected firms are more likely to exclude recurring expenses from non-GAAP earnings and expenses incremental to those excluded by analysts, post-FINSA. We have also shown that the magnitude of these exclusions has increased for FINSA-affected firms and that managers of these firms are more likely to use non-GAAP earnings to beat analysts' expectations. We next test whether these changes in non-GAAP reporting behavior have resulted in lower-quality non-GAAP earnings as measured by earnings persistence. Inspired by Bentley et al. (2018), we test the persistence of non-GAAP earnings by estimating Equation (4):

$$\textit{Future Performance}_{it} = B_0 + B_1\textit{Non-GAAP Earnings}_{it} + B_2\textit{Non-GAAP Earnings}_{it} \times \textit{PostFINSA}_{it} + B_3\textit{Manager Total Exclusions}_{it} + B_4\textit{Manager Total Exclusions}_{it} \times \textit{PostFINSA}_{it} + B_5\textit{PostFINSA}_{it} + B_{6\text{-}16}\textit{Controls}_{it} + \lambda_k + a_t + \varepsilon_{it} \quad (4)$$

where $\textit{Future Performance}_{it}$ is the sum of GAAP operating income over fiscal quarters *t+1* to *t+4*. $\textit{Non-GAAP Earnings}_{it}$ is non-GAAP EPS multiplied by diluted shares outstanding, divided by total assets at the end of quarter *t*. We control for *Size*, *Market-to-Book*, *Big 4 Indicator*, *Standard Deviation of Cash Flows*, *Investor Sentiment*, *Institutional Ownership*, *Number of Analysts Following*, *Book Leverage*, *Repeat non-GAAP Reporter*, *Special Charges*, and *Fourth Quarter Indicator*. $\lambda_k$ is a set of industry fixed effects, $a_t$ is a set of year-quarter fixed effects and standard errors are clustered by firm. Equation (4) is estimated separately for control firms and FINSA-affected firms. If FINSA-affected firms' non-GAAP reporting is more opportunistic, after FINSA, we should expect $\widehat{B_2} < 0$ for FINSA firms.

We report the coefficient estimates of interest from Equation (4) in Table 10. For both control firms and FINSA-affected firms, we see that $\widehat{B_1}$ is significantly positive, suggesting strong persistence of non-GAAP earnings overall. We find a negative coefficient on $\widehat{B_2}$ for FINSA-affected firms and a positive coefficient for control firms. This suggests that the quality of non-GAAP earnings declined for FINSA firms at the same time that it improved for control firms.[24] Overall, we conclude that the changes in non-GAAP reporting behavior

[24] The improvement in non-GAAP earnings quality for control firms is consistent with the consensus in the literature that the informativeness and quality of non-GAAP reporting has improved over time. The decline in non-GAAP earnings quality for treated

of firms facing a weakened takeover market have resulted in lower quality non-GAAP earnings less predictive of future performance.

**Insert Table 10 about here**

### 4.4 Value Relevance of Non-GAAP Earnings

The increase in the magnitude and frequency of expenses excluded from non-GAAP earnings and the reduced persistence of non-GAAP earnings reported by FINSA-affected firms after FINSA suggest that FINSA-affected firms report non-GAAP earnings more opportunistically after FINSA. A salient question is whether investors in FINSA-affected firms recognize that managers' incentives around non-GAAP reporting change after FINSA. We estimate the Ohlson (1995) value relevance model employed by Ribeiro et al. (2018) and Collins et al. (1997) to determine whether investor reliance on non-GAAP earnings changes after FINSA:

$$Price_{it} = B_0 + B_1 Earnings_{it} \times Treatment\ Industry_{it} \times PostFINSA_{it} + B_2 Treatment\ Industry_{it} + B_3 Earnings_{it} + B_4 PostFINSA_{it} + B_5 Treatment\ Industry_{it} \times PostFINSA_{it} + B_6 Earnings_{it} \times Treatment\ Industry_{it} + B_7 Earnings_{it} \times PostFINSA_{it} + B_8 BVE_{it} + B_9 Loss_{it} + B_{10} Earnings_{it} \times Loss_{it} + \varepsilon_{it} \quad (5)$$

where $Earnings_{it}$ is quarter $t$ adjusted non-GAAP earnings in Column (1) and quarter $t$ GAAP earnings in Column (2). All variables are defined in Appendix C. Standard errors are clustered by firm. The results, reported in Column (1) of Table 11, suggest that investors indeed recognize the change in managers' non-GAAP reporting incentives after the decline in the takeover market. The negative coefficient in Column (1) suggests a significant decline in the value relevance of non-GAAP earnings for FINSA-affected firms. We find that $\widehat{B_1}$ is significantly negative for non-GAAP earnings, consistent with investors reducing their reliance on non-GAAP earnings when forming prices after FINSA. We re-estimate Equation (5) using GAAP EPS to determine whether a degradation of the information environment for FINSA-affected firms around FINSA could instead explain this result. We report the result in Column (2) of Table 11. The null result for $\widehat{B_1}$ in Column (2) is inconsistent with this alternative explanation for the result in Column (1). Overall, Table 11 suggests investor awareness of the reduced quality of non-GAAP earnings.

**Insert Table 11 about here**

firms is consistent with our finding of an increased magnitude in recurring expense exclusions post-FINSA, rendering the non-GAAP EPS figure less informative for these firms than in the pre-FINSA period.

**5.0 Additional Tests for Robustness**

The results of sections 4.1 through 4.4 are consistent with managers who are facing a decline in takeover risk engaging in myopic non-GAAP reporting. The tests in these sections produce significant results after factoring for other determinants of non-GAAP reporting documented in the literature. Nonetheless, we perform seven additional robustness checks to investigate the stability of our results.

First, FINSA legislation affects U.S. firms. Potential threats to our inferences are contemporaneous events that affect all firms in critical industries regardless of country. To rule out the presence of a global factor that affects non-GAAP reporting by all firms in critical industries around FINSA, we replicate our test examining whether non-U.S. critical industry firms exclude more recurring expenses from non-GAAP earnings using European Union (EU) data.[25] We present the results in Table 12. For this test, we juxtapose Column (1) of Table 12, which examines EU firms only, with Column (2) of Table 4, which examines U.S. firms only. The test variable in Table 12 is *Placebo Treatment Industry* × *Post-FINSA*. *Placebo Treatment Industry* is equal to one for firms in industries affected by FINSA, and zero otherwise. Of the 1,362 firm-years in the EU sample, approximately one-fifth are in FINSA-affected industries. If a global factor drives the variation in non-GAAP reporting that we observe in U.S. critical industry firms, then we expect to find a positive and statistically significant coefficient on our test variable in the EU sample that mirrors the positive and statistically significant test variable coefficient in Column (2) of Table 4. Instead, we find a negative and statistically insignificant coefficient on our test variable. The sign and significance of the test variable coefficient remains unchanged as we first add industry and year fixed effects in Column (2) of Table 12, and then as we add firm and year fixed effects in Column (3) of Table 12. These results do not support the competing explanation that there was a global shift in non-GAAP reporting by critical industry firms around the enactment of FINSA.

**Insert Table 12 about here**

[25] We follow the method used by Isidro and Marques (2019) to distinguish recurring from non-recurring exclusions in the EU setting. We thank Ana Marques and Helena Isidro for generously sharing their hand-collected EU non-GAAP reporting data.

Second, recognizing that values of some control variables are unevenly distributed across control and FINSA-affected firms, we conduct entropy balancing to better match treated firms with comparable control firms based on size, performance, and leverage. Using this entropy balanced sample, we re-run all our tests.[26] Results from the entropy balancing tests (untabulated) yield the same inferences as our main sample.

Third, we control for year-quarter fixed effects in our tabulated tests. As FINSA had a simultaneous effect on firms covered by its legislation, the *post-FINSA* effect is subsumed by time fixed effects. We re-estimate all our equations with the indicator variable *post-FINSA* included as an independent variable and remove time fixed effects. We find qualitatively similar results, yielding the same inferences.

Fourth, given the timing of the FINSA legislation, it is possible that the 2007-2008 financial crisis may have impacted FINSA-affected firms differently than control firms, and that this impact coincides somewhat with the effect of FINSA. As the first two quarters of 2008 are part of the FINSA legislative period, they are excluded from our analyses. As a robustness check, we also exclude the second half of 2008, which was when the US stock market was in steep decline. Results are qualitatively similar, and inferences are unchanged when this period is removed from the sample.

Fifth, within the non-GAAP literature, there is notable variation in the definition of regulated industries subsequently removed from estimation sample. For example, Bentley et al. (2018) remove finance, insurance and real estate firms (2-digit SIC codes 60-67), whereas Bhattacharya et al. (2018) exclude financial firms and utilities (2-digit SIC codes 49, 60-69). We run all our tests under several different industry restriction choices and consistently find results qualitatively similar to those of our main tests.[27]

Sixth, in 2004, the FASB issued SFAS 123R, which requires recognition of stock-based compensation expense and took effect in 2006. Barth et al. (2012) note that high technology firms were strongly affected by SFAS 123R and were "among the most vocal opponents to recognizing stock-based compensation expense" (Barth et al., 2012). High tech firms represent a significant portion of treated firms in our sample. Thus, it is possible that the increase in recurring expense exclusions and other non-GAAP outcomes we find for treated

[26] See McMullin and Schonberger (2018) for a discussion of the use of entropy balancing in earnings management tests.
[27] Specifically, we attempt the following alternative industry restrictions based on 2-digit SIC codes, all of which have been used in the non-GAAP literature: (1) 67; (2) 60-67; (3) 49, 60-69; (4) no restriction.

firms is driven by high tech firms excluding stock-based compensation, whose requirement for expensing was unrelated to FINSA but was contemporaneous with the start of the FINSA regulatory period.[28] To rule out this explanation for our results, we run our tests after excluding high tech firms. Results are qualitatively similar to those of our main tests.

Finally, we address concerns about firm influence over FINSA legislation. This concern stems from critiques of state-antitakeover laws as a source of variation in the takeover market. The prior literature criticizes exogeneity claims made by those using state-antitakeover laws because many firms are thought to have influenced the antitakeover legislation adopted by the states in which they are incorporated. Gartman (2000) reports that 49 state antitakeover laws in 23 different states were subject to firm lobbying that favored passage. Despite the small number of lobbying firms vis-à-vis the sample sizes used in the prior literature, Karpoff and Wittry (2018) find that retaining these lobbying firms has a material impact on inferences drawn from analyses using state antitakeover laws. We compile data from U.S. Senate records on lobbying activities related to FINSA and show that, of the 31 companies that lobbied for or against FINSA, 13 were U.S. companies. Of these 13 companies, 5 were M&A transaction brokers. Consequently, we replicate our analyses by varying the inclusion of the 13 domestic firms we have identified as having lobbied for or against FINSA. The lobbying firms were Boeing Company, Carlyle Group, Conoco Philips, EDS Corporation, Exxon Mobil, General Electric, Goldman & Sachs, Halliburton, JP Morgan Chase, Lehman Brothers, Merrill Lynch, United Technologies Corporations and Xcel Energy. Inferences drawn after excluding lobbying firms are nearly identical to those drawn from our main specification.

**6.0 Conclusion**

The prior literature debates whether non-GAAP reporting, a form of reporting which permits managers significant discretion, is informative or opportunistic. We add to this debate by examining the effect of an exogenous source of variation in a managerial disciplining mechanism, the takeover market, on non-GAAP reporting quality. We exploit FINSA as a shock to the takeover market based on evidence that FINSA significantly decreased the frequency of takeovers for firms deemed critical to national security (Godsell et al.,

[28] We thank Kurt Gee for bringing this possibility to our attention.

2019). We first posit the many pathways by which the takeover market can determine non-GAAP reporting outcomes stemming from the role of the takeover market as both a managerial disciplining mechanism and as a driver of private information gathering. Using a DiD research design, we find support for the management entrenchment hypothesis that predicts managers will engage in more opportunistic non-GAAP reporting when the threat of takeover declines. We find that firms affected by FINSA exclude more and larger recurring items from non-GAAP performance measures, that these exclusions more often and by larger margins exceed contemporaneous exclusions used in analysts' own estimates of non-GAAP earnings, and that these exclusions are more often used to beat analyst benchmarks when GAAP earnings fall short.

In additional testing, we find that our results accentuate predictably with the likelihood a firm was subject to the threat of takeover in the pre-FINSA period, and the extent to which non-GAAP earnings are used as a determinant of executive compensation. We find in earnings persistence tests that the persistence of non-GAAP earnings in critical industries declines after FINSA. We further find that the value relevance of non-GAAP earnings declines for critical industries, relative to non-critical industries, after FINSA, consistent with investor cognizance of FINSA's weakening of an important managerial disciplining mechanism.

Overall, our results show that financial protectionism has important financial reporting consequences. We draw credible inferences because we are able to benchmark our results against a counterfactual of non-GAAP reporting by U.S. firms in non-critical industries, and against the counterfactual of firms in critical industries but domiciled in the EU and thereby unaffected by FINSA. Our findings contribute to the literature examining determinants of non-GAAP reporting and to the debate regarding whether non-GAAP performance measures are informative or opportunistic. These insights are relevant to non-GAAP scholars, investors, analysts, auditors, board directors and regulators interested in the determinants of non-GAAP reporting quality and to audiences interested in the unintended consequences of regulating foreign investment. For example, our findings should be of interest to legislators in the increasing number of countries that are considering or have recently passed FINSA-like legislation where reporting of non-GAAP earnings is proliferating (e.g., Britain, France, Germany, China, and India [Steinitz and Ingrassia, 2009; CNN Business, 2018]), and in the U.S., where

CFIUS authority has been expanded, under the Foreign Investment Risk Reduction and Management Act of 2018, to include joint venture and greenfield foreign investment (Rose, 2018).

**References**


Badertscher, B. 2011. Overvaluation and the choice of alternative earnings management mechanisms. *The Accounting Review*, 86(5): 1491-1518.

Baker, M. and Wurgler, J. 2006. Investor sentiment and the cross-section of stock returns. *Journal of Finance*, 61 (4): 1645-1680.

Barth, M., Gow, I. and Taylor, D. 2012. Why do pro forma and Street earnings not reflect changes in GAAP? Evidence from SFAS 123R. *Review of Accounting Studies,* 17: 526-562.

Bebchuk, L., Fried, J. and Walker, D. 2002. Managerial power and rent extraction in the design of executive compensation. *The University of Chicago Law Review*, 69: 751-846.

Bentley, J., Christensen, T., Gee, K. and Whipple, B. 2018. Disentangling Managers' and Analysts' Non-GAAP Reporting. *Journal of Accounting Research,* 56(4): 1039-1081.

Bertrand, M. and Mullainathan, S. 2003. Enjoying the quiet life? Managerial behavior following antitakeover legislation. *Journal of Political Economy* 111: 1043–75.

Bhagwat, V., Dam, R. and Harford, J. 2016. The real effects of uncertainty on merger activity. *Review of Financial Studies,* 29(11): 3000-3034.

Bhattacharya, N., Black, E., Christensen, T. and Larson, C. 2003. Assessing the relative informativeness and permanence of pro forma earnings and GAAP operating earnings. *Journal of Accounting and Economics,* 36: 285-319.

Bhattacharya, N., Christensen, T., Liao, Q. and Ouyang, B. 2018. Can Short Sellers Constrain Aggressive Non-GAAP Reporting? Working paper.

Black, D., Black, E., Christensen, T. and Gee, K. 2018a. The Use of Non-GAAP Performance Metrics for Compensation Contracting and Financial Reporting. Working paper.

Black, D., Christensen, T., Ciesielski, J. and Whipple, B. 2018b. Non-GAAP Earnings: A Consistency and Comparability Crisis? Working paper.

Black, D., Christensen, T., Ciesielski, J. and Whipple, B. 2018c. Non-GAAP reporting: Evidence from academia and current practice. *Journal of Business Finance and Accounting,* 45: 259-294.

Black, E., Christensen, T., Kiosse, P. and Steffen, T. 2017a. Has the Regulation of Non-GAAP Disclosures Influenced Managers' Use of Aggressive Earnings Exclusions? *Journal of Accounting, Auditing and Finance,* 32(2): 209-240.

Black, E., Christensen, T., Joo, T. and Schmardebeck, R. 2017b. The Relation Between Earnings Management and Non-GAAP Reporting. *Contemporary Accounting Research,* 34(2): 750-782.

Bonaime, A., Gulen, H. and Ion, M. 2018. Does policy uncertainty affect mergers and acquisitions. *Journal of Financial Economics*, 129 (3): 531-556.

Brown, N., Christensen, T., Elliott, W. and Mergenthaler, R. 2012. Investor Sentiment and Pro Forma Earnings Disclosures. *Journal of Accounting Research,* 50(1): 1-40.

Byrne, M. 2015. Protecting national security and promoting foreign investment: Maintaining the Exon-Florio Balance. *Ohio State Law Journal*, 67: 849-910.

Christensen, T., Drake, M. and Thornock, J. 2014. Optimistic Reporting and Pessimistic Investing: Do Pro Forma Earnings Disclosures Attract Short Sellers? *Contemporary Accounting Research,* 31(1): 67-102.

Christensen, T., Gomez, E., Ma, M. and Pan, J. 2018. Analysts' role in shaping non-GAAP reporting: Evidence from a natural experiment. Working paper.

Collins, D., Maydew, E. and Weiss, I. 1997. Changes in the value-relevance of earnings and book values over the past forty years. *Journal of Accounting and Economics*, 24(1): 39-67.

CNN Business. 2018. July 24. "UK cracks down on foreign investment to protect national security." https://money.cnn.com/2018/07/24/investing/foreign-investment-rules-uk/index.html

Curtis, A., McVay, S. and Whipple, B. 2014. The Disclosure of Non-GAAP Earnings Information in the Presence of Transitory Gains. *The Accounting Review,* 89(3): 933-958.

Doyle, J., Lundholm, R. and Soliman, M. 2003. The Predictive Value of Expenses Excluded from Pro Forma Earnings. *Review of Accounting Studies,* 8: 145-174.

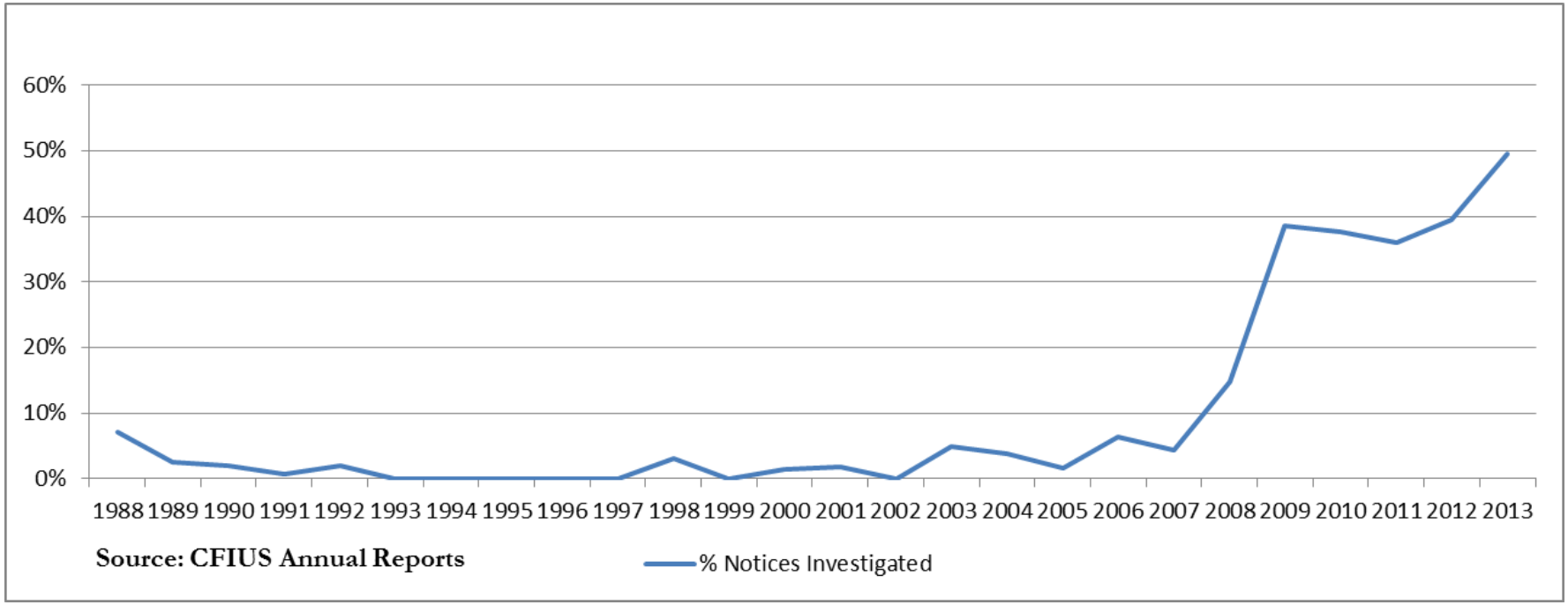
Eckbo, E. 2009. Bidding strategies and takeover premiums: A review. *Journal of Corporate Finance*, 15: 149-178.

Elliott, W. 2006. Are Investors Influenced by Pro Forma Emphasis and Reconciliations in Earnings Announcements? *The Accounting Review,* 81(1): 113-133.
Fama, E. 1980. Agency problem and the theory of the firm. *Journal of Political Economy*, 88: 288-308.
Fama, E. and Jensen, M. 1983. Separation of ownership and control. *Journal of Law and Economics*, 26: 301-325.
Ferreira, M.A., Laux, P.A., 2007. Corporate governance, idiosyncratic risk, and information flow. *Journal of Finance*, 62: 951–989.
Frattaroli, M. 2019. Does protectionist anti-takeover legislation lead to managerial entrenchment? *Journal of Financial Economics*, forthcoming.
Giroud, X., Mueller, H.M., 2010. Does corporate governance matter in competitive industries? *Journal of Financial Economics* 95: 312–331.
Godsell, D. 2019. Does the Threat of Takeover Discipline Managers? New Evidence from the Foreign Investment and National Security Act. Working paper.
Godsell, D., Lel, U., and Miller, D. 2019. Financial Protectionism, M&A Activity, and Shareholder Wealth. Working paper.
Grossman, S. and Hart, O. 1980. Takeover bids, the free-rider problem , and the theory of the corporation. *Bell Journal of Economics*, 11: 42-64.
Guest, N., Kothari, S. and Pozen, R. 2019. High Non-GAAP Earnings Predict Abnormally High CEO Pay. Working paper.
Harford, J. 2003. Takeover bids and target directors' incentives: The impact of a bid on directors' wealth and board seats. *Journal of Financial Economics*, 69: 51-83.
Healy, P. and Wahlen, J. 1999. A review of the earnings management literature and its implications for standard setting. *Accounting Horizons*, 13: 365-383.
Heflin, F. and Hsu, C. 2008. The impact of the SEC's regulation of non-GAAP disclosures. *Journal of Accounting and Economics,* 46: 349-365.
Hirshleifer, D. and Thakor, A. 1998. Corporate control through board dismissals and takeovers. *Journal of Economics and Management Strategy*, 7: 489-520.
Isidro, H. and Marques, A. 2019. The effects of compensation and board quality on non-GAAP disclosures in Europe. *International Journal of Accounting*, 48: 289-317.
Jarrell, G. and Bradley, M. 1980. The economic effects of federal and state regulations of cash tender offers. *Journal of Law and Economics*, 23. 371-407.
Jens, C. 2017. Political uncertainty and investment: Causal evidence from U.S. gubernatorial elections. *Journal of Financial Economics*, 124: 563-579.
Jensen, M. 1993. The modern industrial revolution, exit and the failure of internal control systems. *Journal of Finance*, 48: 831-880.
Karpoff, J., Schnolau, R. and Wehrly, E. 2017. Do takeover defences deter takeovers? *Review of Financial Studies*, 30(7): 2359-2412.
Kolev, K., Marquardt, C. and McVay, S. 2008. SEC Scrutiny and the Evolution of Non-GAAP Reporting. *The Accounting Review,* 83(1): 157-184.
Lel, U. and Miller, D. 2015. Does takeover activity cause managerial discipline? Evidence from international M&A laws. *Review of Financial Studies*, 28(6): 1588-1622.
Leone, M. 2010, September 29. What's on the SEC's radar? *CFO.com*.
Lougee, B. and Marquardt, C. 2004. Earnings Informativeness and Strategic Disclosure: An Empirical Examination of "Pro Forma" Earnings. *The Accounting Review,* 79(3): 769-795.
McMullin, J. and Schonberger, B. 2018. Entropy-balanced accruals. Working paper. Indiana University.
Macey, J. 1988. State antitakeover legislation and the national economy. *Wisconsin Law Review*, 467-490.
Manne, H. 1965. Mergers and the market for corporate control. *Journal of Political Economy*, 73: 110-120.
Moran, T. 2009. Three Threats: An Analytical Framework for the CFIUS Process. Petersen Institute for International Economics.
New York Times. 2006. March 10. "Under pressure, Dubai company drops port deal". https://www.nytimes.com/2006/03/10/politics/under-pressure-dubai-company-drops-port-deal.html

Ohlson, J. 1995. Earnings, book values, and dividends in equity valuation. *Contemporary Accounting Research*, 11(2): 661-687.
Phelps, L. 2019. Filling in the GAAP: A Review and Assessment of Common Practice in Canadian Pro Forma Financial Reporting. Working paper.
Pitt, H. 2001. Remarks before the AICPA governing council. *Miami Beach, FL,* October 22, 2001.
Ribeiro, A., Shan, Y. and Taylor, S. 2018. Non-GAAP Earnings and the Earnings Quality Trade-off. *Abacus*, 55(1): 6-41.
Rose, P. 2014. The Foreign Investment and National Security Act of 2007: An assessment of its impact on Sovereign Wealth Funds and State-Owned Enterprises. Working paper. Ohio State University.
Rose, P. 2018. FIRRMA and National Security. Ohio State Public Law Working Paper #452.
Scharfstein, D. 1988. The disciplinary role of takeovers. *Review of Economic Studies*, 185-199.
Steinitz, M. and Ingrassia, M. The impact of sovereign wealth funds on the regulation of foreign direct investment in strategic industries: A comparative view. *Business International Journal,* 10: 5-18.
Teitelbaum, R. 2015, December 9. Non-GAAP numbers may confuse investors: SEC chair. *The Wall Street Journal.*
Williamson, O. 1983. Organization form, residual claimants and corporate control. *Journal of Law and Economics*, 26: 351-366.
Zang, A. 2012. Evidence on the trade-off between real activities manipulation and accrual-based earnings management. *The Accounting Review*, 87(2): 675-703.
Zhang, H. and Zheng, L. 2011. The valuation impact of reconciling pro forma earnings to GAAP earnings. *Journal of Accounting and Economics,* 51: 186-202.

**Figure 1. CFIUS notices investigated**

This graph shows the percentage of foreign investment notices investigated by the CFIUS. FINSA is enacted in 2008 and fully implemented in 2009.



## Appendix A. Mitigation Measures

Mitigation measures negotiated and adopted between 2008 and 2015 required the businesses involved to take specific and verifiable actions, including, for example:

- Providing the USG with the right to review certain business decisions and object if they raise national security concerns
- Termination of specific activities of the U.S. business
- Ensuring that only authorized persons have access to certain technology and information
- Establishing a Corporate Security Committee and other mechanisms to ensure compliance with all required actions, including the appointment of a USG-approved security officer or member of the board of directors and requirements for security policies, annual reports and independent audits
- Ensuring compliance with established guidelines and terms for handling existing or future USG contracts and USG customer information
- Establishing guidelines and terms for handling existing or future USG contracts, USG customer information and other sensitive information
- Ensuring only U.S. persons handle certain products and services and that certain activities and products are located only in the U.S.
- Notifying security officers or relevant USG parties in advance of foreign national visits to the U.S. business for approval
- Notifying relevant USG parties of any material introduction, modification or discontinuation of a product or service as well as any awareness of any vulnerability or security incidents
- Ensuring continued production of certain products for relevant USG parties for specified periods
- Requiring a proxy entity to perform certain functions and activities of the U.S. business

CFIUS agencies use a variety of means to monitor and enforce compliance by the companies that are subject to the measures, including:

- Increasing USG staffing levels and assigning staff responsibilities for monitoring compliance
- Designing tracking systems to monitor required reports
- Periodic reporting to USG agencies by the companies
- On-site compliance reviews by USG agencies
- Third-party audits when provided for by the terms of the mitigation measures

Investigations and remedial actions if anomalies or breaches are discovered or suspected

## Appendix B. Qualitative Features of National-Security Firms

U.S. businesses that:

1. Provide products and services to an agency or agencies of the USG or state and local authorities that have functions that are relevant to national security

2. Provide products or services that could expose national security vulnerabilities, including potential cyber security concerns, or create vulnerability to sabotage or espionage (This includes consideration of whether the covered transaction will increase the risk of exploitation of the particular U.S. business's position in the supply chain.)

3. Have operations, or produce or supply products or services, the security of which may have implications for U.S. national security, such as businesses that involve infrastructure that may constitute critical infrastructure; businesses that involve various aspects of energy production, including extraction, generation, transmission and distribution; businesses that affect the national transportation system; and businesses that could significantly and directly affect the U.S. financial system

4. Have access to classified information or sensitive government or government contract information, including information about employees

5. Are in the defense, security and national security-related law enforcement sectors

6. Are involved in activities related to weapons and munitions manufacturing, aerospace, satellite and radar systems

7. Produce certain types of advanced technologies that may be useful in defending or in seeking to impair U.S. national security, which may include businesses engaged in the design and production of equipment or components that have both commercial and military applications (Such transactions have included, for example, businesses engaged in the design, production or provision of goods and services involving network and data security. They have also included businesses that produce semiconductor manufacturing equipment, design integrated circuits and fabricate integrated circuits in light of the fact that semiconductors are an enabling technology for a range of national security critical devices, systems and functions. They have also included businesses that are in the biotechnology sector given the potential military applications of such technology and the sensitivity of the information such companies may collect.)

8. Engage in the research and development, production or sale of technology, goods, software or services that are subject to U.S. export controls

9. Are in a field with significant national security implications in which there are few alternative suppliers or in which a loss in U.S. technological competitiveness would be detrimental to national security

10. Have operations or facilities that are in proximity to military or other sensitive USG facilities

11. Hold substantial pools of potentially sensitive data about U.S. persons and businesses that have national security importance (Such businesses could be in any number of sectors, including, for example, the insurance sectors, health services and technology services.)

**Appendix C. Variable Definitions**

| Variable Name | Definition | Data Source |
|---|---|---|
| Treatment Industry | A firm-level variable equal to one for firms in national security-related industries and zero otherwise. | CFIUS Annual Report |
| Post-FINSA | A variable equal to one in years after 2008 and zero otherwise. | CFIUS Annual Report |
| Non-GAAP EPS | Manager-reported non-GAAP earnings per share. | Bentley et al. (2018) Non-GAAP EPS data. |
| GAAP EPS | Earnings before extraordinary items divided by total common shares outstanding. | CRSP / Compustat |
| Size | Size is equal to the log of total assets. | CRSP / Compustat |
| ROA | ROA is equal to income before extraordinary items divided by total assets at the beginning of the fiscal quarter. | CRSP / Compustat |
| Market-to-Book | Market-to-Book is equal to market value of equity divided by book equity. | CRSP / Compustat |
| Big 4 Indicator | Big 4 Indicator is equal to one if the firm is audited by a Big 4 auditor, zero otherwise. | CRSP / Compustat |
| Standard Deviation of Cash Flows | Standard deviation of annual cash flows deflated by lagged total assets over the past three years. | CRSP / Compustat |
| Investor Sentiment | Investor Sentiment is equal to the average level of the monthly investor sentiment index of Baker and Wurgler (2006) over the earnings announcement quarter. | Jeffrey Wurgler's website: people.stern.nyu.edu/jwurgler |
| Institutional Ownership | Percentage of total firm equity held by institutional investors. | Thomson Reuters |
| Number of Analysts Following | Number of analyses following the firm. | I/B/E/S |
| Book Leverage | Book Leverage is the log of liabilities scaled by book equity. | CRSP / Compustat |
| Repeat non-GAAP Reporter | Repeat non-GAAP Reporter is equal to one if the firm reported non-GAAP EPS in the previous quarter, zero otherwise. | Bentley et al. (2018) Non-GAAP EPS data. |
| Special Charges | Special Charges is equal to one if the firm reported non-zero income-decreasing special items in the current quarter, zero otherwise. | CRSP / Compustat |
| Fourth Quarter Indicator | Fourth Quarter Indicator is equal to one if the observation is the fourth fiscal quarter of the year, zero otherwise. | CRSP / Compustat |
| **Outcome Variables** | | |
| Manager Total Exclusions Indicator | Manager Total Exclusions Indicator is a variable equal to one if Manager Total Exclusions is greater than zero, zero otherwise. | CRSP / Compustat and Bentley et al. (2018) Non-GAAP EPS data. |
| Manager Recurring Exclusions Indicator | Manager Recurring Exclusions Indicator is a variable equal to one if Manager Recurring Exclusions is greater than zero, zero otherwise. | CRSP / Compustat and Bentley et al. (2018) Non-GAAP EPS data. |
| Manager Non-Recurring Exclusions Indicator | Manager Non-Recurring Exclusions Indicator is a variable equal to one if Manager Non-Recurring Exclusions is greater than zero, zero otherwise. | CRSP / Compustat and Bentley et al. (2018) Non-GAAP EPS data. |
| Manager Total Exclusions | Manager Total Exclusions is a variable equal to non-GAAP EPS minus GAAP EPS after extraordinary items, scaled by assets per share. | CRSP / Compustat and Bentley et al. (2018) Non-GAAP EPS data. |

| | | |
|---|---|---|
| Manager Recurring Exclusions | Manager Recurring Exclusions is a variable equal to non-GAAP EPS minus GAAP EPS from operations, scaled by assets per share. | CRSP / Compustat and Bentley et al. (2018) Non-GAAP EPS data. |
| Manager Non-Recurring Exclusions | Manager Non-Recurring Exclusions is a variable equal to GAAP EPS from operations minus GAAP EPS after extraordinary items, scaled by assets per share. | CRSP / Compustat and Bentley et al. (2018) Non-GAAP EPS data. |
| Manager Incremental Exclusions Indicator | Manager Incremental Exclusions Indicator is a variable equal to one if Manager Incremental Exclusions is greater than zero, zero otherwise. | I/B/E/S, CRSP / Compustat and Bentley et al. (2018) Non-GAAP EPS data. |
| Manager Incremental Exclusions | Manager Incremental Exclusions is a variable equal to non-GAAP EPS minus I/B/E/S actual EPS, scaled by assets per share. | I/B/E/S, CRSP / Compustat and Bentley et al. (2018) Non-GAAP EPS data. |
| StreetBeat | StreetBeat is a variable equal to one if non-GAAP EPS meets or beats the analysts' consensus EPS forecast but GAAP EPS from operations misses the analysts' consensus GAAP EPS forecast. | I/B/E/S, CRSP / Compustat and Bentley et al. (2018) Non-GAAP EPS data. |

| Table 1: Sample Construction | | |
|---|---|---|
| This table reports the construction of the estimation panel. | | |
| Firm-quarter observations with non-GAAP reporting disclosures in our sample period | | 15,767 |
| Less: Firms with missing test and control variables | (2,993) | 12,774 |
| Less: Firms without at least one pre and post FINSA observation | (5,835) | 6,939 |
| Less: Firms in regulated industries | (839) | 6,100 |
| **Estimation Sample Size:** | | **6,100** |

**Table 2: Sample Breakdown by Industry and Year**

This table reports sample characteristics of the control and treated firms.

**Panel A: Industry Breakdown**

| | FINSA (Treatment) Firm-Years | | Non-FINSA (Control) Firm-Years | |
|---|---|---|---|---|
| **Fama-French industry code 10 industries)** | **Frequency** | **%** | **Frequency** | **%** |
| Non-Durable | 0 | 0.0% | 384 | 11.9% |
| Consumer Durable | 6 | 0.2% | 133 | 4.1% |
| Manufacturing | 114 | 4.0% | 832 | 25.8% |
| Energy | 24 | 0.8% | 353 | 11.0% |
| High Tech | 1,977 | 68.7% | 484 | 15.0% |
| Telecommunication | 0 | 0.0% | 0 | 0.0% |
| Wholesale Retail | 0 | 0.0% | 464 | 14.4% |
| Health | 736 | 25.6% | 115 | 3.6% |
| Utilities | 0 | 0.0% | 0 | 0.0% |
| Other (Mines, Construction) | 20 | 0.7% | 458 | 14.2% |
| **Total** | **2,877** | | **3,223** | |

**Panel B: Year Breakdown**

| | FINSA (Treatment) Firm-Years | | Non-FINSA (Control) Firm-Years | |
|---|---|---|---|---|
| **Year** | **Freq.** | **%** | **Freq.** | **%** |
| 2003 Q1 | 55 | 1.7% | 64 | 2.2% |
| 2003 Q2 | 77 | 2.4% | 74 | 2.6% |
| 2003 Q3 | 100 | 3.1% | 89 | 3.1% |
| 2003 Q4 | 115 | 3.6% | 104 | 3.6% |
| 2004 Q1 | 83 | 2.6% | 72 | 2.5% |
| 2004 Q2 | 97 | 3.0% | 87 | 3.0% |
| 2004 Q3 | 115 | 3.6% | 93 | 3.2% |
| 2004 Q4 | 115 | 3.6% | 102 | 3.5% |
| 2005 Q1 | 88 | 2.7% | 108 | 3.8% |
| 2005 Q2 | 121 | 3.8% | 111 | 3.9% |
| 2005 Q3 | 133 | 4.1% | 113 | 3.9% |
| 2005 Q4 | 148 | 4.6% | 123 | 4.3% |
| … | | | | |
| 2008 Q3 | 163 | 5.1% | 146 | 5.1% |
| 2008 Q4 | 205 | 6.4% | 169 | 5.9% |
| 2009 Q1 | 167 | 5.2% | 154 | 5.4% |
| 2009 Q2 | 176 | 5.5% | 157 | 5.5% |
| 2009 Q3 | 169 | 5.2% | 146 | 5.1% |
| 2009 Q4 | 181 | 5.6% | 153 | 5.3% |
| 2010 Q1 | 140 | 4.3% | 136 | 4.7% |
| 2010 Q2 | 157 | 4.9% | 145 | 5.0% |
| 2010 Q3 | 157 | 4.9% | 139 | 4.8% |
| 2010 Q4 | 175 | 5.4% | 136 | 4.7% |
| 2011 Q1 | 133 | 4.1% | 123 | 4.3% |
| 2011 Q2 | 153 | 4.7% | 133 | 4.6% |
| **Total** | **3,223** | | **2,877** | |

**Table 3: Sample Summary Statistics**

Sample summary statistics are presented below. All variables are defined in Appendix C.

| | Control Firms | | | | Treatment Firms | | | | Difference in Means t-statistic |
|---|---|---|---|---|---|---|---|---|---|
| **Test Variables** | | | | | | | | | |
| Manager Total Exclusions Indicator | 0.77 | 1.00 | 0.42 | 3,215 | 0.87 | 1.00 | 0.33 | 2,878 | -10.30 |
| Manager Recurring Exclusions Indicator | 0.61 | 1.00 | 0.49 | 3,215 | 0.81 | 1.00 | 0.39 | 2,878 | -17.20 |
| Manager Non-Recurring Exclusions Indicator | 0.59 | 1.00 | 0.49 | 3,215 | 0.56 | 1.00 | 0.50 | 2,878 | 2.70 |
| Manager Total Exclusions | 0.01 | 0.00 | 0.03 | 3,215 | 0.02 | 0.01 | 0.04 | 2,878 | -6.00 |
| Manager Recurring Exclusions | 0.00 | 0.00 | 0.01 | 3,215 | 0.01 | 0.00 | 0.01 | 2,878 | -14.30 |
| Manager Non-Recurring Exclusions | 0.01 | 0.00 | 0.03 | 3,215 | 0.01 | 0.00 | 0.03 | 2,878 | -1.10 |
| Manager Incremental Exclusions Indicator | 0.11 | 0.00 | 0.32 | 3,215 | 0.11 | 0.00 | 0.31 | 2,878 | 0.40 |
| Manager Incremental Exclusions | 0.00 | 0.00 | 0.00 | 3,215 | 0.00 | 0.00 | 0.00 | 2,878 | -2.60 |
| StreetBeat | 0.22 | 0.00 | 0.41 | 3,215 | 0.44 | 0.00 | 0.50 | 2,878 | -19.51 |
| **Other Variables** | | | | | | | | | |
| | **Mean** | **Median** | **St. Dev.** | **N** | **Mean** | **Median** | **St. Dev.** | **N** | Difference in Means t-statistic |
| Non-GAAP EPS | 0.41 | 0.33 | 0.45 | 3,223 | 0.29 | 0.23 | 0.36 | 2,877 | 11.30 |
| GAAP EPS | 0.20 | 0.25 | 0.86 | 3,223 | 0.12 | 0.12 | 0.62 | 2,877 | 4.40 |
| Size | 5888.45 | 1830.70 | 14696.22 | 3,223 | 4427.75 | 959.28 | 13859.24 | 2,877 | 12.60 |
| ROA | 0.08 | 0.08 | 0.08 | 3,223 | 0.06 | 0.07 | 0.10 | 2,877 | 11.00 |
| Market-to-Book | 2.56 | 1.85 | 2.69 | 3,223 | 2.88 | 2.32 | 2.35 | 2,877 | -5.00 |
| Big 4 Indicator | 0.94 | 1.00 | 0.24 | 3,223 | 0.93 | 1.00 | 0.25 | 2,877 | 0.90 |
| Standard Deviation of Cash Flows | 0.04 | 0.03 | 0.03 | 3,223 | 0.05 | 0.03 | 0.04 | 2,877 | -11.00 |
| Investor Sentiment | -0.29 | -0.43 | 0.37 | 3,223 | -0.29 | -0.43 | 0.37 | 2,877 | 0.10 |
| Institutional Ownership | 0.76 | 0.81 | 0.19 | 3,223 | 0.76 | 0.81 | 0.20 | 2,877 | 0.15 |
| Number of Analysts Following | 9.88 | 8.00 | 6.99 | 3,223 | 11.28 | 9.00 | 8.01 | 2,877 | -7.30 |
| Book Leverage | 0.55 | 0.56 | 0.20 | 3,223 | 0.40 | 0.38 | 0.20 | 2,877 | 29.80 |
| Repeat Non-GAAP Reporter | 0.57 | 1.00 | 0.50 | 3,223 | 0.69 | 1.00 | 0.46 | 2,877 | -10.20 |
| Special Charges | 0.63 | 1.00 | 0.48 | 3,223 | 0.65 | 1.00 | 0.48 | 2,877 | -1.30 |
| Fourth Quarter Indicator | 0.29 | 0.00 | 0.45 | 3,223 | 0.27 | 0.00 | 0.45 | 2,877 | 1.50 |

**Table 4: The effect of a reduction in the threat of takeover on Manager Exclusion Frequency**

This table examines a non-GAAP variable in a DiD setup. Treatment Industry is equal to one for firms affected by FINSA, and zero otherwise. Post-FINSA is equal to one in year-quarters after Q2 2008 and zero otherwise. Manager Total Exclusions Indicator is a variable equal to Non-GAAP EPS minus GAAP EPS after extraordinary items. Manager Recurring Exclusions Indicator is a variable equal to Non-GAAP EPS minus GAAP EPS from operations. Manager Non-Recurring Exclusions Indicator is a variable equal to GAAP EPS from operations minus GAAP EPS after extraordinary items. All other variables are defined in Appendix C. Standard errors are clustered by firm. Two-tailed t-statistics are presented underneath the coefficient estimates. ***, **, and * denote two-tailed significance levels at 1%, 5%, and 10%, respectively. The model is estimated via a logistic regression.

| | (1) | (2) | (3) |
|---|---|---|---|
| | Manager Total Exclusions Indicator | Manager Recurring Exclusions Indicator | Manager Non-Recurring Exclusions Indicator |
| Treatment Industry | 0.0662 | 0.173* | -0.0486 |
| | (0.63) | (1.89) | (-0.47) |
| **Treatment Industry × Post-FINSA** | **0.215**** | **0.345***** | **-0.0645** |
| | **(2.12)** | **(3.79)** | **(-0.61)** |
| Size | -0.103*** | -0.0254 | 0.0436 |
| | (-2.85) | (-0.80) | (1.32) |
| ROA | -1.814*** | -1.798*** | -0.619** |
| | (-4.85) | (-5.43) | (-2.11) |
| Market-to-Book | -0.00377 | 0.00537 | -0.0298*** |
| | (-0.38) | (0.61) | (-3.06) |
| Big 4 Indicator | 0.0591 | -0.0219 | 0.0202 |
| | (0.55) | (-0.22) | (0.18) |
| Standard Deviation of Cash Flows | -1.911*** | -0.857 | -0.732 |
| | (-2.98) | (-1.41) | (-1.13) |
| Investor Sentiment | -0.0307 | 0.160 | -0.383** |
| | (-0.17) | (1.19) | (-2.12) |
| Institutional Ownership | -0.0363 | -0.105 | 0.129 |
| | (-0.23) | (-0.78) | (0.84) |
| Number of Analysts Following | 0.0209*** | 0.0180*** | -0.00917 |
| | (2.89) | (3.03) | (-1.56) |
| Book Leverage | -0.244 | -0.253* | 0.349** |
| | (-1.34) | (-1.69) | (2.28) |
| Repeat Non-GAAP Reporter | 0.177*** | 0.187*** | -0.105** |
| | (3.61) | (4.62) | (-2.14) |
| Special Charges | 1.130*** | -0.0781* | 2.640*** |
| | (19.79) | (-1.72) | (39.98) |
| Fourth Quarter Indicator | -0.302* | -0.139 | 0.280 |
| | (-1.69) | (-0.96) | (1.50) |
| Number of Observations | 6088 | 6091 | 6090 |
| Standard Errors Clustered By: | Firm | Firm | Firm |
| Industry Fixed Effects: | Yes | Yes | Yes |
| Year × Quarter Fixed Effects: | Yes | Yes | Yes |

**Table 5: The effect of a reduction in the threat of takeover on Manager Exclusion Magnitudes**

This table examines a non-GAAP variable in a DiD setup. Treatment Industry is equal to one for firms affected by FINSA, and zero otherwise. Post-FINSA is equal to one in year-quarters after Q2 2008 and zero otherwise. Manager Total Exclusions is a variable equal to Non-GAAP EPS minus GAAP EPS after extraordinary items, scaled by assets per share. Manager Recurring Exclusions is a variable equal to Non-GAAP EPS minus GAAP EPS from operations, scaled by assets per share. Manager Non-Recurring Exclusions is a variable equal to GAAP EPS from operations minus GAAP EPS after extraordinary items, scaled by assets per share. All other variables are defined in Appendix C. Standard errors are clustered by firm. Two-tailed t-statistics are presented underneath the coefficient estimates. ***, **, and * denote two-tailed significance levels at 1%, 5%, and 10%, respectively.

| | (1) | (2) | (3) |
|---|---|---|---|
| | Manager Total Exclusions | Manager Recurring Exclusions | Manager Non-Recurring Exclusions |
| Treatment Industry | 0.00101 | 0.000745 | 0.0000914 |
| | (0.56) | (0.92) | (0.07) |
| **Treatment Industry × Post-FINSA** | **0.00113** | **0.00206*** ** | **-0.000727** |
| | **(0.68)** | **(3.17)** | **(-0.57)** |
| Size | -0.00321*** | -0.00111*** | -0.00199*** |
| | (-5.65) | (-4.41) | (-5.07) |
| ROA | -0.0795*** | -0.0359*** | -0.0316*** |
| | (-8.08) | (-8.23) | (-4.87) |
| Market-to-Book | -0.000128 | 0.000112 | -0.000271 |
| | (-0.45) | (0.84) | (-1.59) |
| Big 4 Indicator | 0.00261 | -0.000878 | 0.00362** |
| | (1.09) | (-0.79) | (2.17) |
| Standard Deviation of Cash Flows | 0.0149 | 0.0142** | -0.000840 |
| | (0.82) | (2.05) | (-0.08) |
| Investor Sentiment | -0.0168*** | -0.00329** | -0.0143*** |
| | (-4.34) | (-2.42) | (-4.68) |
| Institutional Ownership | -0.00191 | -0.00302** | 0.000175 |
| | (-0.63) | (-2.46) | (0.09) |
| Number of Analysts Following | 0.000335*** | 0.000207*** | 0.000111* |
| | (3.76) | (5.05) | (1.76) |
| Book Leverage | 0.00913** | 0.000889 | 0.00694*** |
| | (2.50) | (0.55) | (3.10) |
| Repeat Non-GAAP Reporter | -0.00289*** | 0.000560* | -0.00318*** |
| | (-3.00) | (1.65) | (-4.26) |
| Special Charges | 0.0126*** | 0.000355 | 0.0124*** |
| | (15.53) | (1.01) | (19.55) |
| Fourth Quarter Indicator | 0.0176*** | 0.00477*** | 0.0128*** |
| | (5.17) | (3.84) | (4.79) |
| Number of Observations | 6093 | 6093 | 6093 |
| Adjusted R-Squared | 0.184 | 0.179 | 0.158 |
| Standard Errors Clustered By: | Firm | Firm | Firm |
| Industry Fixed Effects: | Yes | Yes | Yes |
| Year × Quarter Fixed Effects: | Yes | Yes | Yes |

**Table 6: The effect of a reduction in the threat of takeover on the frequency of Management Exclusions that exceed Analyst Exclusions**

This table examines a non-GAAP variable in a DiD setup. Treatment Industry is equal to one for firms affected by FINSA, and zero otherwise. Post-FINSA is equal to one in year-quarters after Q2 2008 and zero otherwise. Manager Incremental Exclusions Indicator is a variable equal to one if Manager Incremental Exclusions is greater than zero, zero otherwise. Standard errors are clustered by firm. Standard errors are clustered by firm. Two-tailed t-statistics are presented underneath the coefficient estimates. ***, **, and * denote two-tailed significance levels at 1%, 5%, and 10%, respectively. All other variables are defined in Appendix C. The model is estimated via a logistic regression.

| | (1) |
|---|---|
| | Manager Incremental Exclusions Indicator |
| Treatment Industry | -0.309** |
| | (-2.18) |
| **Treatment Industry × Post-FINSA** | **0.277**** |
| | **(2.12)** |
| Size | -0.171*** |
| | (-3.67) |
| ROA | -0.229 |
| | (-0.46) |
| Market-to-Book | 0.00213 |
| | (0.14) |
| Big 4 Indicator | -0.0548 |
| | (-0.36) |
| Standard Deviation of Cash Flows | 0.921 |
| | (0.98) |
| Investor Sentiment | 0.235 |
| | (1.53) |
| Institutional Ownership | 0.144 |
| | (0.64) |
| Number of Analysts Following | -0.000834 |
| | (-0.10) |
| Book Leverage | -0.0332 |
| | (-0.15) |
| Repeat Non-GAAP Reporter | -0.0610 |
| | (-1.14) |
| Special Charges | -0.259*** |
| | (-3.53) |
| Fourth Quarter Indicator | 0.236 |
| | (1.12) |
| Number of Observations | 5909 |
| Standard Errors Clustered By: | Firm |
| Industry Fixed Effects: | Yes |
| Year × Quarter Fixed Effects: | Yes |

**Table 7: The effect of a reduction in the threat of takeover on the magnitude of Management Exclusions that exceed Analyst Exclusions**

This table examines a non-GAAP variable in a DiD setup. Treatment Industry is equal to one for firms affected by FINSA, and zero otherwise. Post-FINSA is equal to one in year-quarters after Q2 2008 and zero otherwise. Manager Incremental Exclusions is a variable equal to Non-GAAP EPS minus I/B/E/S actual EPS, scaled by assets per share. All other variables are defined in Appendix C. Standard errors are clustered by firm. Two-tailed t-statistics are presented underneath the coefficient estimates. ***, **, and * denote two-tailed significance levels at 1%, 5%, and 10%, respectively.

| | (1) |
|---|---|
| | Manager Incremental Exclusions |
| Treatment Industry | -0.000706* |
| | (-1.78) |
| **Treatment Industry × Post-FINSA** | **0.000731**** |
| | **(2.11)** |
| Size | -0.000500*** |
| | (-3.83) |
| ROA | -0.00481** |
| | (-2.31) |
| Market-to-Book | 0.0000689 |
| | (1.39) |
| Big 4 Indicator | -0.000254 |
| | (-0.47) |
| Standard Deviation of Cash Flows | 0.00646* |
| | (1.87) |
| Investor Sentiment | 0.000191 |
| | (0.35) |
| Institutional Ownership | -0.000550 |
| | (-0.78) |
| Number of Analysts Following | 0.0000347* |
| | (1.92) |
| Book Leverage | 0.000416 |
| | (0.61) |
| Repeat Non-GAAP Reporter | -0.000131 |
| | (-1.00) |
| Special Charges | 0.0000202 |
| | (0.11) |
| Fourth Quarter Indicator | 0.000582 |
| | (1.37) |
| Number of Observations | 6093 |
| Adjusted R-Squared | 0.054 |
| Standard Errors Clustered By: | Firm |
| Industry Fixed Effects: | Yes |
| Year × Quarter Fixed Effects: | Yes |

**Table 8: The effect of a reduction in the threat of takeover on the use of Non-GAAP to shift an analyst consensus miss to a beat**

This table examines a non-GAAP variable in a DiD setup. Treatment Industry is equal to one for firms affected by FINSA, and zero otherwise. Post-FINSA is equal to one in year-quarters after Q2 2008 and zero otherwise. StreetBeat is a variable equal to one if Non-GAAP EPS meets or beats the analysts' consensus EPS forecast but GAAP EPS from operations misses the analysts' consensus GAAP EPS forecast. All other variables are defined in Appendix C. Standard errors are clustered by firm. Two-tailed t-statistics are presented underneath the coefficient estimates. ***, **, and * denote two-tailed significance levels at 1%, 5%, and 10%, respectively. The model is estimated via a logistic regression.

| | (1) |
|---|---|
| | StreetBeat |
| Treatment Industry | 0.140 |
| | (1.32) |
| **Treatment Industry × Post-FINSA** | **0.471*** ** |
| | **(5.05)** |
| Size | 0.0374 |
| | (1.11) |
| ROA | -1.742*** |
| | (-5.51) |
| Market-to-Book | -0.00111 |
| | (-0.11) |
| Big 4 Indicator | -0.0232 |
| | (-0.19) |
| Standard Deviation of Cash Flows | -0.269 |
| | (-0.42) |
| Investor Sentiment | 0.0917 |
| | (0.69) |
| Institutional Ownership | 0.240 |
| | (1.59) |
| Number of Analysts Following | 0.0174*** |
| | (3.04) |
| Book Leverage | -0.410** |
| | (-2.49) |
| Repeat Non-GAAP Reporter | 0.224*** |
| | (5.27) |
| Special Charges | -0.126*** |
| | (-2.80) |
| Fourth Quarter Indicator | -0.0688 |
| | (-0.43) |
| Number of Observations | 6050 |
| Standard Errors Clustered By: | Firm |
| Industry Fixed Effects: | Yes |
| Year × Quarter Fixed Effects: | Yes |

**Table 9: Cross-sectional Analyses**

This table examines a non-GAAP variable in a DiD setup after partitioning the sample by a cross-sectional variable. The first cross-sectional variable is the pre-FINSA likelihood of takeover. The second cross-sectional variable is the sensitivity of manager compensation to non-GAAP earnings. Treatment Industry is equal to one for firms affected by FINSA, and zero otherwise. Post-FINSA is equal to one in year-quarters after Q2 2008 and zero otherwise. Standard errors are clustered by firm. Two-tailed t-statistics are presented underneath the coefficient estimates. ***, **, and * denote two-tailed significance levels at 1%, 5%, and 10%, respectively. Columns 1, 2, 5, 6, 9 and 10 estimate logistic regressions.

**Panel A: Takeover Probability Cross-Sectional Analysis**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | **Manager Recurring Exclusions Indicator** | | **Manager Recurring Exclusions** | | **Manager Incremental Exclusions Indicator** | | **Manager Incremental Exclusions** | | **StreetBeat** | |
| | Low Takeover Probability | High Takeover Probability | Low Takeover Probability | High Takeover Probability | Low Takeover Probability | High Takeover Probability | Low Takeover Probability | High Takeover Probability | Low Takeover Probability | High Takeover Probability |
| **Treatment Industry × Post-FINSA** | 0.212 | 0.474*** | 0.00174* | 0.00237*** | 0.141 | 0.396* | 0.000626 | 0.000742 | 0.456*** | 0.530*** |
| | (1.64) | (3.62) | (1.69) | (2.91) | (0.83) | (1.90) | (1.13) | (1.47) | (3.59) | (3.75) |
| Number of Observations | 3041 | 3046 | 3047 | 3046 | 2948 | 2908 | 3047 | 3046 | 2988 | 2954 |
| Adjusted R-Squared | | | 0.163 | 0.187 | | | 0.058 | 0.041 | | |
| Standard Errors Clustered By: | Firm | Firm | Firm | Firm | Firm | Firm | Firm | Firm | Firm | Firm |
| Control Variables: | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry Fixed Effects: | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year × Quarter Fixed Effects: | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

**Panel B: Sensitivity of Compensation to Non-GAAP Earnings Cross-Sectional Analysis**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | **Manager Recurring Exclusions Indicator** | | **Manager Recurring Exclusions** | | **Manager Incremental Exclusions Indicator** | | **Manager Incremental Exclusions** | | **StreetBeat** | |
| | Low Sensitivity | High Sensitivity | Low Sensitivity | High Sensitivity | Low Sensitivity | High Sensitivity | Low Sensitivity | High Sensitivity | Low Sensitivity | High Sensitivity |
| **Treatment Industry × Post-FINSA** | 0.339** | 0.346*** | -0.0000507 | 0.00253*** | 0.150 | 0.367** | -0.000251 | 0.00120** | 0.426** | 0.434*** |
| | (2.05) | (2.93) | (-0.05) | (2.85) | (0.77) | (2.08) | (-0.66) | (2.32) | (2.45) | (3.54) |
| Number of Observations | 1967 | 3560 | 1967 | 3560 | 1849 | 3560 | 1967 | 3560 | 1967 | 3560 |
| Adjusted R-Squared | | | 0.174 | 0.173 | | | 0.083 | 0.055 | | |
| Standard Errors Clustered By: | Firm | Firm | Firm | Firm | Firm | Firm | Firm | Firm | Firm | Firm |
| Control Variables: | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry Fixed Effects: | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year × Quarter Fixed Effects: | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

**Table 10: Persistence Tests**

This table examines the persistence of Non-GAAP earnings after FINSA for treatment and control industry firms. Post-FINSA is equal to one in year-quarters after Q2 2008 and zero otherwise. All other variables are defined in Appendix C. Standard errors are clustered by firm. Two-tailed t-statistics are presented underneath the coefficient estimates. ***, **, and * denote two-tailed significance levels at 1%, 5%, and 10%, respectively.

| | (1) | (2) |
|---|---|---|
| | Future Earnings | |
| | Control Industry | Treatment Industry |
| Non-GAAP Earnings | 2.355*** | 2.978*** |
| | (12.27) | (13.61) |
| **Non-GAAP Earnings × Post-FINSA** | **0.361*** | **-0.556**** |
| | **(1.71)** | **(-2.02)** |
| Managerial Exclusions | -0.163*** | -0.174** |
| | (-3.25) | (-2.53) |
| Managerial Exclusions × Post-FINSA | -0.0123 | -0.0275 |
| | (-0.13) | (-0.23) |
| Size | 0.00189 | 0.00687*** |
| | (1.06) | (3.05) |
| Market-to-Book | 0.00418*** | 0.00340** |
| | (4.88) | (2.29) |
| Big 4 Indicator | 0.00327 | 0.0156 |
| | (0.45) | (1.42) |
| Standard Deviation of Cash Flows | 0.0544 | -0.0522 |
| | (0.72) | (-0.74) |
| Investor Sentiment | -0.0178** | -0.0390*** |
| | (-2.58) | (-3.58) |
| Institutional Ownership | 0.0284*** | 0.0498*** |
| | (3.03) | (3.30) |
| Number of Analysts Following | 0.000220 | 0.000100 |
| | (0.63) | (0.26) |
| Book Leverage | -0.00287 | -0.0289** |
| | (-0.24) | (-2.01) |
| Repeat Non-GAAP Reporter | -0.00470** | -0.00289 |
| | (-2.51) | (-0.89) |
| Special Charges | -0.0000435 | -0.00173 |
| | (-0.02) | (-0.43) |
| Fourth Quarter Indicator | 0.00279 | -0.00813 |
| | (0.40) | (-0.65) |
| Number of Observations | 3170 | 2808 |
| Adjusted R-Squared | 0.566 | 0.555 |
| Standard Errors Clustered By: | Firm | Firm |
| Industry Fixed Effects: | Yes | Yes |
| Year × Quarter Fixed Effects: | Yes | Yes |

**Table 11: Value Relevance Tests**

This table examines the value-relevance of non-GAAP and GAAP earnings after FINSA. Price is equal to the average price around the earnings announcement date (–5, +5). Earnings is equal to either Non-GAAP or GAAP earnings as indicated by the column header. Equity book value is common shareholder's equity divided by the total number of shares outstanding. Loss is an indicator variable equal to one if the income before extraordinary items is less than zero, and zero otherwise. All other variables are defined in Appendix C. Standard errors are clustered by firm. Two-tailed t-statistics are presented underneath the coefficient estimates. ***, **, and * denote two-tailed significance levels at 1%, 5%, and 10%, respectively.

| | (1) | (2) |
|---|---|---|
| | Price | |
| | Non-GAAP Earnings | GAAP Earnings |
| **Earnings × Treatment Industry × Post-FINSA** | **-12.39*** ** | **-1.168** |
| | **(-3.38)** | **(-0.67)** |
| Treatment Industry | -0.415 | 4.046*** |
| | (-0.38) | (3.92) |
| Earnings | 22.24*** | 20.44*** |
| | (10.05) | (12.73) |
| Post-FINSA | -1.609** | 0.778 |
| | (-2.01) | (1.18) |
| Treatment Industry × Post-FINSA | -1.736 | -4.305*** |
| | (-1.40) | (-4.09) |
| Earnings × Treatment Industry | 14.73*** | 2.548*** |
| | (4.33) | (2.65) |
| Earnings × Post-FINSA | 7.096*** | 1.158 |
| | (3.73) | (1.38) |
| Equity Book Value | 0.738*** | 0.982*** |
| | (10.02) | (14.93) |
| Loss | 0.0237 | -3.326*** |
| | (0.03) | (-5.37) |
| Loss × Earnings | -18.80*** | -21.24*** |
| | (-5.94) | (-12.70) |
| Number of Observations | 6076 | 6076 |
| Adjusted R-Squared | 0.605 | 0.549 |
| Standard Errors Clustered By: | Firm | Firm |

**Table 12: Placebo Test using European Union (EU) Firms**

This table examines the frequency of manager recurring exclusions in a placebo group of FINSA industries in the EU which should be unaffected by FINSA. Placebo Treatment Industry is equal to one for firms in industries affected by FINSA, but domiciled in the EU, and zero for firms in industries unaffected by FINSA and domiciled in the EU. Post-FINSA is equal to one in year-quarters after Q2 2008 and zero otherwise. Manager Recurring Exclusions Indicator is a variable equal to one if the firm excludes recurring items in Non-GAAP earnings. Standard errors are clustered by firm. Two-tailed t-statistics are presented underneath the coefficient estimates. ***, **, and * denote two-tailed significance levels at 1%, 5%, and 10%, respectively. The model is estimated via a logistic regression.

| | (1) | (2) | (3) |
|---|---|---|---|
| | Manager Recurring Exclusions Indicator | | |
| Placebo Treatment Industry | 0.501** | 2.397*** | 0.136 |
| | (2.31) | (6.27) | (0.52) |
| Post-FINSA | -1.423*** | -0.724* | -1.311* |
| | (-6.10) | (-1.90) | (-1.95) |
| **Placebo Treatment Industry × Post-FINSA** | **-0.282** | **-0.556** | **-0.590** |
| | **(-0.64)** | **(-0.76)** | **(-0.50)** |
| Number of Observations | 1362 | 508 | 275 |
| Standard Errors Clustered By: | Firm | Firm | Firm |
| Industry Fixed Effects: | No | Yes | No |
| Firm Fixed Effects: | No | No | Yes |
| Year Fixed Effects: | No | Yes | Yes |